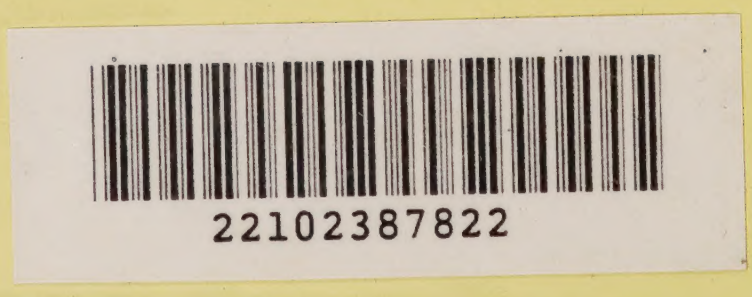
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TRANSACTIONS

OF

THE MEDICAL SOCIETY

OF

LONDON.

VOLUME THE FOURTEENTH.

EDITED BY

THOMAS COLCOTT FOX, M.B.,

AND

A. MARMADUKE SHEILD, F.R.C.S.

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1. From and after the day on which this Scheme is approved and established by an Order of the Charity Commissioners, the above-mentioned Charity and the endowment specified in the Schedule hereto, and all other endowments (if any) of the said Charity, shall be administered and managed by the council of the Medical Society of London, hereinafter called the Trustees, subject to and in conformity with the provisions of this Scheme, under the title of the Fothergillian Fund.

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N.B.—The first award of the Medal under the new Scheme will be made in March, 1893.

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„ Henry Hancock, On the Anatomy and Physiology of the Male Urethra, and on the Pathology of Stricture of that Canal.

1854. Edward William Murphy, M.D., On Parturition as Illustrating the Importance of a Competent Education in the Practice of Midwifery.

1855. Theophilus Thompson, M.D., On Pulmonary Consumption.

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„ Francis Sibson, M.D., F.R.S., On the Influence of the Nervous System on Respiration and Circulation.

„ Francis Hird, On some Special Points in the Anatomy of the Uterus, and its Structural Lesions the result of Inflammation.

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„ John Louis William Thudichum, M.D., On Medicine: the Progress of Urology, with Practical Illustrations of its Value in the Diagnosis and Treatment of several Diseases.

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1799. Edward Ford.
1800. Thomas Bradley, M.D.
1801. William Chamberlaine.
1802. John Sims, M.D.
1803. John Andrée.
1804. John Coakley Lettsom, M.D., F.R.S.
1805. George Pinckhard, M.D.
1806. Henry Field.
1807. Joseph Adams, M.D.
1808. John Mason Good, F.R.S.
1809. Sayer Walker, M.D.
1810. George Birkbeck, M.D.
1811. William Blair.
1812. Richard Temple, M.D.
1813. Richard Saumarez, F.R.S.
1814. George Rees, M.D.
1815. John Taunton.
1816. Henry Clutterbuck, M.D.
1817. James Stevenson.
1818. David Uwins, M.D.
1819. Thomas J. Pettigrew, F.R.S.
1820. Thomas Hancock, M.D.
1821. Thomas Callaway.
1822. James Copland, M.D.
1823. Edward Grainger.
1824. Gordon Smith, M.D.
1825. Eusebius Arthur Lloyd.
1826. John Haslam, M.D.
1827. William Kingdom.
1828. John Burne, M.D.
1829. William Greville Jones.
1830. Leonard Stewart, M.D.
1831. Montague Gossett.
1832. John Whiting, M.D.
1833. Frederick Salmon.
1834. William Shearman, M.D.
1835. Walter Cooper Dendy.
1836. William F. Blicke, M.D.
1837. Edward Headland.
1838. Theophilus Thompson, M.D., F.R.S.
1839. George Pilcher.
1840. James Risdon Bennett, M.D.
1841. Wm. Dingle Chowne, M.D.
1842. Henry Hancock.
1843. Leonard Stewart, M.D.
1844. Thomas Bell, F.R.S.
1845. Marshall Hall, M.D.
1846. John Bishop, F.R.S.
1847. Golding Bird, M.D., F.R.S.
1848. Francis Hird.
1849. William Hughes Willshire, M.D.
1850. Francis Hird.
1851. Richard Rowland.
1852. Edwin Canton.
1853. John Snow, M.D.
1854. Henry Smith.
1855. James Fernandez Clarke.
1856. Benj. Ward Richardson, M.D., F.R.S.
1857. William Adams.
1858. Alfred Baring Garrod, M.D.
1859. Charles Henry Felix Routh, M.D.

1860. John Gay.
1861. Arthur Leared, M.D.
1862. Victor de Méric.
1863. Samuel Osborne Habershon, M.D.
1864. John Louis William Thudichum, M.D.
1865. Robert Greenhalgh, M.D.
1866. Thomas Christopher Weeden Cooke.
1867. Frederick William Headland, M.D.
1868. William Frederick Teevan.
1869. George Duncan Gibb, M.D.
1870. Francis Mason.
1871. William Cholmeley, M.D.
1872. Frederick James Gant.
1873. John Cockle, M.D.
1874. Robert Brudenell Carter.
1875. George Buchanan, M.D.
1876. Erasmus Wilson, F.R.S.
1877. John Hughlings Jackson, M.D., F.R.S.
1878. Alfred Carpenter, M.D.
1879. Walter John Coulson.
1880. William Henry Broadbent, M.D.
1881. Arthur Edward Durham.
1882. Edmund Symes Thompson, M.D.
1883. Edward Lund.
1884. Charles Theodore Williams, M.D.
1885. George Murray Humphry, M.D., F.R.S.
1886. Richard Douglas Powell, M.D.
1887. Sir William MacCormac, F.R.C.S.
1888. Sir Joseph Fayrer, K.C.S.I., M.D., F.R.S.
1889. Jonathan Hutchinson, F.R.S.
1890. Arthur Ernest Sansom, M.D.
1891. Sir Joseph Lister, Bart., F.R.S.

THE FOTHERGILLIAN GOLD MEDALLISTS.

1787. William Falconer, M.D.
1790. Robert Willan, M.D.
1791. John Coakley Lettsom, M.D.
1795. John Mason Good.
1801. Francis Bouttatz, M.D.
1803. Edward Jenner, M.D.
1824. Robert W. Bampfield.
1828. John George Parry.
1831. William Augustus Guy.
1834. William James Clement.
1835. George Moore.
1836. Thomas Egerton Bryant.
1838. George Pilcher.
1840. Samuel Osborn.
1842. James Risdon Bennett, M.D.
1843. John Weaver Lever, M.D.
1844. Henry Pratt Robarts.
1845. Walter Cooper Dendy.
1846. Robert Mortimer Glover, M.D.
1847. Silas Stedman.
1849. John Milligan.
1850. Richard Payne Cotton, M.D.
1851. Richard Hodges.
1852. Frederick William Headland.
1853. Alfred William Poland.
1854. Benjamin Ward Richardson, M.D.
1856. William Burke Ryan.
1857. Edwin Canton.
1858. Thomas Herbert Barker, M.D.
1859. Alderman Thomas Houghton Waters.
1868. John Clay.
1870. Thos. Smith Clouston, M.D.
1872. Edwards Crisp, M.D.
1873. John Kent Spender, M.D.
1877. Peter Murray Braidwood, M.D.
1878. John Milner Fothergill, M.D.
1882. Thomas Michael Dolan, M.D.
1883. Norman Porritt.
1886. John Strahan.
1888. Hobart Amory Hare, M.D., U.S.A.

THE HONORARY FELLOWS.

1876. Barnes, J. K., M.D., Surgeon-General U.S. Army, Washington.

1877. Bennett, Sir James Risdon, LL.D., M.D., F.R.S., 22, Cavendish-square, W., late President of the Royal College of Physicians; Consulting Physician to St. Thomas's Hospital and to the Victoria Park Hospital. P 2, VP 2, C 7, O, S 3, FM.

1881. Billings, John S., M.D., Washington, Surgeon to the United States Army; Librarian to the Surgeon-General's Library, Washington.

1881. Billroth, Theodore, M.D., Professor of Surgery in the University of Vienna.

1881. Charcot, Professor J. M., M.D., Physician to the Hôpital de la Salpêtrière, and Professor of the Faculty of Medicine, Paris.

1873. Chauveau, A., Professor of Physiology at the Medical School of Lyons.

1890. Crudeli, Tommasi, M.D., Rome.

1881. Da Costa, J. M., M.D., Professor of Medicine in the Jeffreson Medical College, 1700, Walnut-street, Philadelphia.

1881. Emmet, Thomas Addis, M.D., 89, Madison-avenue, Surgeon to the Woman's Hospital of the State of New York.

1886. Gairdner, William Tennant, M.D., LL.D. Edin., F.R.C.P. Edin.

1881. Halla, Joseph, Professor of Medicine in the University of Prague.

1869. Hare, Charles John, M.D., Manchester-square, W., Emeritus Professor of Clinical Medicine in University College; Consulting Physician to University College Hospital. P, VP 2. C 8, LL. *Trustee.*

1873. Helmholtz, Hermann Ludwig Ferdinand, M.D., Professor of Physics and Physiological Optics in the University of Berlin.

1890. Holmgren, Frithiof, Professor, Upsala.

1883. Humphry, Sir George Murray, M.D., F.R.S., Professor of Surgery in the University of Cambridge. O, C 2.

1873. Huxley, Thomas Henry, LL.D., F.R.S., 4, Marlborough-place, St. John's Wood, N.W., Professor of Biology in the Normal School of Science and in the Royal School of Mines.

1875. Jenner, Sir William, Bart., K.C.B., D.C.L., LL.D., M.D., F.R.S., Greenwood, Durley, Hants, Physician-in-Ordinary to H.M. the Queen and to H.R.H. the Prince of Wales; late President of the Royal College of Physicians; Emeritus Professor of Clinical Medicine in University College, London; Consulting Physician to University College Hospital.

1890. Kocher, Theodor, Professor, Berne.
1884. Larrey, Baron, M.D., Paris, Rue de Lille, 91.
1883. Le Roy de Mericourt, A., M.D., Paris.
1890. Lombard, Henri-Clermond, M.D., Geneva.
1878. Mitchell, S. Weir, M.D., Walnut-street, Philadelphia.
1881. Nussbaum, John Nepomuk Ritter von, M.D., Professor of Surgery in the University of Munich.
1875. Ollier, Professor, Lyons.
1873. Owen, Sir Richard, K.C.B., F.R.S., Sheen Lodge, Richmond Park, Superintendent of the Natural History Department of the British Museum.
1873. Paget, Sir James, Bart., D.C.L., LL.D., F.R.S., 1, Harewood-place, Hanover-square, W., Serjeant-Surgeon to H.M. the Queen; Surgeon to H.R.H. the Prince of Wales; Consulting Surgeon to St. Bartholomew's Hospital.
1883. Paget, Sir George Edward, D.C.L., LL.D., M.D., F.R.S., Regius Professor of Physic in the University of Cambridge.
1876. Pancoast, Joseph, M.D., 1030, Chesnut-street, Philadelphia, Professor of Anatomy in the Jeffreson Medical College.
1890. Pasteur, Louis, Member of the Institute of France, Paris.
1877. Sanné, A., 12, Place de Laborde, Paris.
1835. Seaone, M., M.D., Salamanca.
1881. Tarnier, Stephanie, M.D., Professor of Obstetric Medicine in the School of Medicine, Paris.
1873. Tyndall, John, F.R.S.
1881. Verneuil, Auguste Aristide, M.D., Professor of Medicine in the School of Medicine, Paris.
1873. Virchow, Rudolph, M.D., Professor of Pathological Anatomy in the University of Berlin.

CORRESPONDING FELLOWS.

1851. Albaro, J. Mendez, Madrid.
1882. Badaloni, Giuseppe, M.D., Fano, Prov. di Pesaro, Italy.
1856. Baker, Albert, M.D., Ottery St. Mary, Devonshire.
1855. Beardsley, Amos, Bay Villa, Grange, Lancashire.
1850. Benavente, Mariano, Madrid.
Beneke, F. W., M.D., New York.
1850. Böhm, Professor, M.D., Vienna.
Bottini, Giuseppe, M.D., Milan.
1837. Buhring, J. J., M.D., Berlin.
1874. Burness, Alexander George, M.D., Port Elizabeth, Cape of Good Hope.
Cade, Thomas Charles, Spondon, Derby.
1855. Coates, Charles, M.D., F.R.C.P., 10, Circus, Bath, Consulting Physician to the Bath Royal United Hospital. c 3.
1850. Cox, William Isidore, Hawkesbury, Upton, Gloucestershire. c.
1876. Dawes, Richard, St. Mark, Gawler, South Australia.
De Muynck, J., M.D., Ghent.
1836. Ecstein, Sigismund, M.D., Vienna.
Eylandt, Johann Emil, M.D., Curland, Russia.
1853. Fallot, R., M.D., St. Laurent d'Aigonze, Montpelier, France.
1889. Frank, Philip, M.D., F.R.C.P., Cannes, France.
1876. Griffith, Richard Glyn, Allahabad, India.
1864. Hasenfeld, Emmanuel, M.D., Pesth.
Hyman, —, M.D., Antwerp.
1851. Izguierdo, Sebastian Obtega, Madrid.
1875. Jones, Philip Sydney, M.D., F.R.C.S., Examiner in Medicine in the University of Sydney, Australia, Hon. Consulting Surgeon to the Sydney Infirmary.
1861. Journez, Henri, M.D., 43, Rue de la Charité, Bruxelles, Belgique.
1851. Kölliker, Albert, M.D., Professor of Anatomy and Physiology at the University of Wurzburg.
1876. Leighton, Walter H., M.D., Lowell, Massachusetts, U.S.
Leon, Jose, Madrid.
1851. Llanos, Antonio Campo, Madrid.
1851. Lovera, Jose, Madrid.

1851. Marino, Bonifacio Matreos, Madrid.
Mendez, Barthoeome, Madrid.
Molina, M. M., Madrid.
Negri, Gaetano, M.D., Pisa.
Ortega, J. R., Madrid.
1865. Peruzzi, Domenico, M.D., 22, Via Mazzini, Bologna.
1882. Restrepo, Alessando Eduardo, M.D., Medillin, Columbia, U.S.A.
1886. Rocha, A., M.D., Coimbra, Beira, Portugal.
1860. Roussel, M.D., Dean of the Faculty of Medicine, Montpelier.
Scharlan, Gus. W., M.D., Stettin, Prussia.
1876. Schmitz, Richard, M.D., Neuenahr.
1874. Schutgowsky, J., St. Petersburg.
1851. Sesse, M., Mesqui, Madrid.
Stockwell, Thomas Goldesbrough, F.R.C.S., 6, Circus, Bath, Surgeon to the Bath Royal United Hospital.
Tereza, Felix Garcia, Madrid.
Valdez, Franco Cortigo, Madrid.
Williams, Charles, F.R.C.S. Edin., 48, Prince of Wales-road, Norwich; Surgeon to the Norfolk and Norwich Hospital.

THE FELLOWS

OF

THE MEDICAL SOCIETY OF LONDON.

Explanation of Abbreviations.

P.—President.	FM.—Fothergillian Gold Medallist.
VP.—Vice-President.	SM.—Silver Medallist.
T.—Treasurer.	O.—Orator.
L.—Librarian.	CFC.—Chairman, House and Finance Committee.
S.—Secretary.	§—Sec. for Foreign Correspondence.
C.—Councillor.	*—Life Members.
LL.—Lettsomian Lecturer.	Tr.—Trustee.

The number prefixed signifies the date of election. The figures appended indicate the number of Sessions served, and refer to past appointments ONLY.

1890. Abbot-Anderson, William Maurice, M.B., 10, Old Cavendish-street, Cavendish-square, W.
1888. Abbott, Charles Edward, M.R.C.S., Noel House, Braintree, Essex.
1891. Abraham, Phineas S., M.D., 2, Henrietta-street, Cavendish-square, W.
1890. Ackland, Robert Craig, M.R.C.S., 13, Savile-row, W.
1883. Acland, Theodore Dyke, M.D., 74, Brook-street, Grosvenor-square, W.
1884. Adam, James, M.D., Malling-place, West Malling, Kent.
1889. Adams, James, M.D., 4, Chiswick-place, Eastbourne.
1878. Adams, Josiah Oake, M.D., Brook House, Upper Clapton, E.
1852. *Adams, William, F.R.C.S., 5, Henrietta-street, Cavendish-square, W. P, C 8, O, VP 3, LL.
1878. *Allchin, William Henry, M.B., F.R.S.E., 5, Chandos-street, Cavendish-square, W. *Vice-President, Hon. Librarian.*
1873. Allen, Henry Marcus, F.R.C.P. Edin., 20, Regency-square, Brighton.
1873. Allfrey, Charles Henry, M.D., Plas Newydd, St. Leonards-on-Sea.
1883. Allingham, Herbert W., F.R.C.S., 25, Grosvenor-street, W. C 2. *Councillor.*
1872. *Allingham, William, F.R.C.S., 25, Grosvenor-street, W. C.
1860. Althaus, Julius, M.D., 48, Harley-street, W. C 5, § 3.

1887. Anderson, James, M.D., 41, Wimpole-street, W.
1885. Anderson, John, M.D., C.I.E., 105, Gloucester-place, W.
1889. Anderson, William, F.R.C.S., 2, Harley-street, W.
1888. Andrewes, Frederick William, M.B., 15, Upper Brook-street, W.
1869. Armitage, Samuel Harris Tatham, M.D., 39, Grosvenor-street, W.
1873. Atkinson, Edward, M.R.C.S., 36, Albion-street, Leeds.
1872. Aveling, James Hobson, M.D., 1, Upper Wimpole-street, W.

1873. Bagshawe, Frederic, M.D., 5, Warrior-square, St. Leonards. *Councillor.*
1871. Bailey, George Hewlett, M.R.C.S., 9, Cavendish-place, W.
1876. *Baker, Henry Francis, F.R.C.S. Edin., 2, Mandeville-place, Manchester-square, W. c.
1890. Baker, William Henry, M.R.C.S., 40, Norfolk-terrace, Bayswater, W.
1879. Balkwill, William Edward, M.R.C.S., 14, Old Cavendish-street, Cavendish-square.
1881. Ballance, Charles Alfred, M.S., 56, Harley-street, W. s 2. *Councillor.*
1884. Banks, W. Mitchell, F.R.C.S., 38, Rodney-street, Liverpool.
1859. Barnes, John Wickham, F.R.C.S., 3, Bolt-court, E.C. s 2, vp, c 3.
1883. Barnes, Robert, M.D., 7, Queen Anne-street, W. ll, c.
1874. Barrett, Howard, M.R.C.S., 3, Tavistock-square, W.C.
1884. Barrow, Albert Boyce, F.R.C.S., 17, Welbeck-street, W.
1886. Barwell, Richard, F.R.C.S., 55, Wimpole-street, W.
1884. Bateman, Frederick Augustus Newton, M.R.C.S., 4, Charles-street, St. James's-street, S.W.
1886. Batterham, John Williams, M.B., Bank House, Grand-parade, St. Leonards-on-Sea.
1888. Battle, William Henry, F.R.C.S., 6, Harley-street, W.
1882. Beach, Fletcher, M.B., Darenth Asylum, Dartford, Kent. c.
1887. Beale, Edwin Clifford, M.B., 23, Upper Berkeley-street, W.
1880. Beevor, Charles Edward, M.D., 33, Harley-street, W. s 2, c. *Trustee.*
1889. Beevor, Sir Hugh Reeve, Bart., M.B., King's College-chambers, Strand, W.C.
1887. Benham, Frederick Lucas, M.D., 93, Elizabeth-street, Eaton-square, S.W.
1881. Bennet, Robert Othwell-Gifford, M.D., Tankerville House, Park-place, Buxton.
1883. Bennett, William Henry, F.R.C.S., 1, Chesterfield-street, Mayfair, W.
1887. Berry, James, F.R.C.S., 60, Welbeck-street, W.
1873. Beveridge, James Spowart, M.R.C.P. Edin. (address uncommunicated).
1890. Bidwell, Leonard Arthur, M.R.C.S., 21, Bentinck-street, Cavendish-square, W.

1868. Bird, George, M.D., 49, Welbeck-street.
1888. Bird, Matthew Mitchell, M.D., Durham, St. Mary's Hospital, W.
1850. *Birkett, John, F.R.C.S., 62, Green-street, Grosvenor-square, W. vp, c 6.
1883. Biss, Cecil Yates, M.D., 135, Harley-street, W.
1889. Bisshopp, Francis Robert Bryant, M.B., Belvedere, Lonsdale-gardens, Tunbridge Wells.
1881. Black, James, F.R.C.S., Hillside, 1, Hermitage-road, Norwood, S.E. c 2.
1886. *Black, William Gall, F.R.C.S., 2, George-square, Edinburgh.
1885. Blake, John Ffrench, Terrace House, Camberwell-green, S.E.
1881. Blaker, Walter Campbell, Bognor, Sussex.
1888. Blanc, Leon, M.D., Aix les Bains, France.
1871. *Bloxam, John Astley, F.R.C.S., 8, George-street, Hanover-square, W. v.p 2, s 2, c 2. *Councillor.*
1867. Bond, Thomas, F.R.C.S., 7, The Sanctuary, Westminster, S.W. c.
1883. Boon, Alfred, F.R.C.S., Basse Terre, St. Kitts, W.I.
1879. Bott, Henry, M.R.C.S., Brentford, Middlesex.
1872. Boulton, Percy, M.D., 6, Seymour-street, Portman-square, W. c.
1886. Bourns, Newcome Whitelaw, M.D., 449, Fulham-road, S.W.
1886. Boustead, Robinson, M.D. (Brigade Surgeon), 10, Palmeira-avenue, West Brighton.
1889. Bowles, Robert Leamon, M.D., 8, West-terrace, Folkestone.
1887. Bradford, Arthur, L.R.C.P., Oakley, Newbridge-road, Weston, Bath.
1883. Bradshaw, James Dixon, M.B., 30, George-street, Hanover-square, W.
1884. Brailey, William Arthur, M.D., 11, Old Burlington-street, W.
1869. Braine, Francis Woodhouse, F.R.C.S., 56, Maddox-street, W. vp 2, s 2, c 3, sm.
1889. Braine, C. Carter, F.R.C.S., 56, Maddox-street, W.
1876. Brewer, Alexander Hampton, 136, Richmond-road, Dalston, E.
1873. Bridgwater, Thomas, M.B., Harrow, Middlesex.
1887. Bristowe, John Syer, M.D., F.R.S., 13, Old Burlington-street, W.
1862. Broadbent, William Henry, M.D., 34, Seymour-street, Portman-square, W. p, vp, o, ll, c 4.
1890. Brook, William Frederick, F.R.C.S., 79, Lambeth Palace-road, S.E.
1879. Brookfield, John Storrs, M.D., 2, Devonshire-villas, Brondesbury, N.W.
1878. Brooks, Job Edwin, 54, Mill-street, Ludlow, Salop.
1878. Brown, Andrew, M.D., Elton Villa, 1, Bartholomew-road, Kentish Town, N.W.
1871. Brown, John, Belmont Lodge, St. John's-hill, New Wandsworth, S.W.
1889. Browne, George Buckston, M.R.C.S., 80, Wimpole-street, W.
1871. Browne, Sir James Crichton, M.D., F.R.S., 7, Cumberland-terrace, Regent's-park, N.W. c. *Orator.*

1873. Browne, Lennox, F.R.C.S. Edin., 36, Weymouth-street, W.
1887. Bruce, John Mitchell, M.D., 70, Harley-street, W.
1873. Brunjes, Martin, M.R.C.S., 33a, Gloucester-place, Bryanston-square, W.
1862. Brunton, John, M.D., 21, Euston-road, N.W. vp, c 2.
1874. *Brunton, Thomas Lauder, M.D., F.R.S., 10, Stratford-place, W. ll, vp, c 4, sm.
1850. *Bryant, Thomas, F.R.C.S., 65, Grosvenor-street, W., President of the Royal College of Surgeons. p, vp, ll, s 2, c 4. *Trustee.*
1858. Buchanan, George, M.D., F.R.S., 27, Woburn-square, W.C. p, ll, vp, o, c 3.
1885. Bull, William Charles, F.R.C.S., 35, Clarges-street, Mayfair, W.
1883. Bull, William Henry, St. Oswald's House, Stony Stratford, Bucks.
1885. *Bunny, J. Brice, M.R.C.S., Newbury, Berks.
1872. Burger, Alexander, M.D., 49, Finsbury-square, E.C.
1890. Butler, Patrick, L.K.Q.C.P., 22, Duke-street, Portland-place, W.
1886. Butler-Smythe, Albert Charles, F.R.C.S., 76, Brook-street, W.
1872. Byas, Edward Hegley, M.R.C.S., 10, Cambridge-gate, Regent's-park, N.W.

1886. Cahill, John, F.R.C.S., 12, Seville-street, Lowndes-square, Hyde Park, S.W.
1888. Campbell, Charles M., M.D., 37, Queen Anne-street, W.
1890. Cardew, Harry Warnell Denton, M.R.C.S., 53, Harley-street, W.
1889. Carnall, Edward, M.R.C.S., 9, Gerrard-street, W.
1869. Carpenter, Alfred, M.D., J.P., Duppas House, Croydon. o, c 3.
1882. Carpenter, Arthur Bristowe, M.B., 34, Dingwall-road, Croydon.
1889. Carr, John Walter, M.D., 40, Bloomsbury-square, W.C.
1871. Carter, Robert Brudenell, F.R.C.S., 27, Queen Anne-street, W., p, vp, o, ll, c 4.
1889. Cartwright, Alexander, M.R.C.S., 32, Old Burlington-street, W.
1876. Cartwright, S. Hamilton, 45, Albert-gate, Hyde-park, W.
1878. Cassidy, Joseph Lamont, M.D., 44, Harley-street, W.
1876. *Cathcart, Samuel, M.R.C.P. Edin., Prudhoe House, High-road, Tottenham, E.
1889. Cautley, Edmund, M.B., 15, Upper Brook-street, W.
1882. Cavafy, John, M.D., 2, Upper Berkeley-street, W. c 3.
1867. Chapman, John, M.D., Avenue Kleber 46, Paris.
1885. Chasseaud, William, M.D., Smyrna, Asia Minor.
1889. Cheyne, Watson, F.R.C.S., 59, Welbeck-street, W. *Councillor.*
1877. *Chisholm, Edwin, M.D., Sydney, New South Wales.
1870. Christie, Thomas Beath, M.D., F.R.S.E., C.I.E., Royal India Asylum, Ealing, W.
1871. Churton, Thomas, M.D., 35, Park-square, Leeds. *Councillor.*

1854. Clark, Sir Andrew, Bart., M.D., F.R.S., 16, Cavendish-square, W., President of the Royal College of Physicians. P, VP, LL, C 5, § 6.
1875. Clark, Andrew, F.R.C.S., 71, Harley-street, W.
1873. Clarke, Thomas Kilner, F.R.C.S., 66, John William-street, Huddersfield.
1883. Clarke, William Bruce, F.R.C.S., 46, Harley-street, W. C.
1879. *Clutton, Henry Hugh, F.R.C.S., 2, Portland-place, W. C 2.
1849. *Cockle, John, M.D., 8, Suffolk-street, Pall Mall, S.W. P, VP, O, L 3, C 3, SM.
1848. *Cogswell, Charles, M.D., F.L.S., 47, York-terrace, Regent's Park, N.W. S 4. Tr 18.
1887. Collier, William, M.D., 62, High-street, Oxford.
1883. Compton, Francis Charles, 72, High-street, Poole, Dorset.
1871. Cook, John, M.D., 1, Nottingham-terrace, Regent's Park, N.W.
1862. Cooper, Alfred, F.R.C.S., 9, Henrietta-street, Cavendish-square, W. C 3, VP.
1888. Cooper, Arthur, 20, Old Burlington-street, W.
1872. Corfield, William Henry, M.D., 19, Savile-row. C.
1888. Cotes, Charles Edward Henry, F.R.C.S., 42, Davies-street, W.
1879. Coupland, Sidney, M.D., 16, Queen Anne-street, W. C 2.
1889. Courtney, Guy Budd, M.B., 47, Seymour-street, W.
1874. Craigie, John Hamilton, 13, Savile-row, W. C.
1873. Craven, Robert Martin, F.R.C.S., J.P., 14, Albion-street, Hull.
1889. Crawford James, M.D., 4, Iddesleigh-mansions, Caxton-street, S.W.
1881. Cripps, William Harrison, F.R.C.S., 2, Stratford-place, W. C 2.
1880. Critchett, George Anderson, F.R.C.S. Edin., 21, Harley-street, W.
1880. Crocker, Henry Radcliffe, M.D., 121, Harley-street, C 3.
1881. Cross, Francis Richardson, F.R.C.S., Chandos Villa, Clifton, Bristol.
1884. Cross, Horatio Robert Odo, Surgeon, Grenadier Guards.
1855. Cross, Robert, M.D., 19, Berkeley-square, W.
1881. Cullimore, Daniel Henry, M.D., 27, Welbeck-street, W.
1890. Cullingworth, Charles James, M.D., 46, Brook-street, W. *Councillor.*
1874. Cumberbatch, Alphonso Elkin, F.R.C.S., 17, Queen Anne-street, W. C 2.

1871. Dalby, Sir William Bartlett, F.R.C.S., 18, Savile-row, W. C.
1864. Dale, George Cornelius, M.D., Ivy Lodge, Upper Tooting, S.W.
1881. Dallaway, Dennis Joseph William, L.R.C.P. Edin., 5, Duchess-street, Portland-place, W.
1873. Daly, Owen, M.D., J.P., 23, Albion-street, Hull.
1885. Davies-Colley, John Neville Colley, F.R.C.S., 36, Harley-street, W. C 3.
1890. Davis, Henry, M.R.C.S., 60, Queen Anne-street, W.
1889. *Davison, James, M.D., Walderslow, Bournemouth.
1880. Davson, Smith Houston, M.D., Campden Villa, 203, Maida-vale, W. C 3.

1868. *Davy, Richard, F.R.C.S., F.R.S.E., 33, Welbeck-street, W. vp, s 2, sm, § 2.
1876. Dawes, H. St. M., M.R.C.S., Gawler, South Australia.
1880. Dawson, Yelverton, M.D., Heathland, Southborne-on-Sea, Christchurch, Hants.
1867. Day, William Henry, M.D., 10, Manchester-square, W. c 3.
1883. Dent, Clinton Thomas, F.R.C.S., 61, Brook-street, Grosvenor-square, W. c 3.
1881. Dickson, Francis Kennedy, F.R.C.P. Edin., Wye House Lunatic Asylum, Buxton, Derbyshire.
1891. Diver, Ebenezer, M.D., Yately House, Kenley, Surrey.
1885. Dodd, Henry Work, F.R.C.S., 47, Kensington Park-gardens, W.
1882. Dolan, Thomas Michael, M.D., Horton House, Halifax. fm 1882.
1881. Doran, Alban Henry Griffiths, F.R.C.S., 9, Granville-place, W. c 2. *Councillor.*
1890. Douglas, William, M.D., Dalkeith House, 7, Clarendon-place, Leamington Spa.
1872. Down, John Langdon Haydon, M.D., 81, Harley-street, W. vp, ll.
1871. Dowse, Thomas Stretch, M.D., 14, Welbeck-street, W. § 3, c 3.
1885. Drew, Edwin, M.D. (Surg.-Major), 32, Holland-park, W.
1877. Drew, John Henry, M.R.C.S., 48, Upper Berkeley-street, W. c 6.
1881. Drewitt, Frederic George Dawtrey, M.D. 42, Brook-street, W.
1874. Drysdale, Charles Robert, M.D., 23, Sackville-street, W.
1886. Duckworth, Sir Dyce, M.D., 11, Grafton-street, Piccadilly, W.
1848. *Duncan, James, M.B., 8, Henrietta-street, Covent Garden, W.C.
1884. Duncan, W., M.D., 6, Harley-street, W.
1873. *Durham, Arthur Edward, F.R.C.S., 82, Brook-street, W. p, o, c. *Treasurer.*
1884. Durham, Frederick, F.R.C.S., 82, Brook-street, W.

1891. Eastes, Thomas, M.D., 3, Shakespeare-terrace, Folkestone.
1860. Edmunds, James, M.D., 8, Grafton-street, W.
1880. Edwards, Frederick Swinford, F.R.C.S., 93, Wimpole-street, W.
1868. Elliott, George Frederick, M.D., 1, Albion-street, Hull.
1882. Elliott, Thomas, M.D., Monson-place, Tunbridge Wells.
1889. Embleton, Dennis Cawood, M.R.C.S., St. Wilfrid's, Bournemouth.
1883. Emond, E., M.D., 113, Boulevard Beaumarchais, Paris.
1883. English, Edgar, M.R.C.S., High-street, Mexborough, near Rotherham.
1880. English, Thomas Johnston, M.D., 128, Fulham-road, S.W.
1889. Esler, Robert, M.D., 4, Queen's-road, Peckham, S.E.
1883. Ewart, Joseph, M.D., J.P., Retired Dep. Surgeon-General, Bengal Army, Montpellier Hall, Brighton.
1877. Ewart, William, M.D. Cantab, 33, Curzon-street, Mayfair, W. c.

1889. Fairbank, Frederick Royston, M.D., 46, Hall-gate, Doncaster.
1884. Fardon, Edward Ashby, M.R.C.S., Middlesex Hospital, W.
1873. Fayrer, Sir Joseph, K.C.S.I., LL.D., M.D., F.R.S., 53, Wimpole-street, W. P, VP, LL, SM, O, C.
1884. Fenton, Frederick Enos, F.R.C.S., The Cedars, Ealing, W.
1888. Fenwick, Bedford, M.D., 20, Upper Wimpole-street, W.
1885. Fenwick, Edwin Hurry, F.R.C.S., 5, Old Burlington-street, W.
1887. Ferrier, David, M.D. Edin., F.R.S., 34, Cavendish-square, W.
1878. Field, George, F.R.C.S., 34, Wimpole-street, W.
1883. Finlay, David White, M.D., 9, Lower Berkeley-street, W. C 2.
1876. Fisher, Frederic Richard, F.R.C.S., 58, Harley-street, W.
1884. Flint, Arthur, M.D., Westgate Lodge, Westgate-on-Sea.
1878. *Fonmartin, Henry de, M.D., 1, Anchor-gate-terrace, Portsea, Hants.
1884. Fotherby, Henry Arthur, 20, Warwick-street, Kensington, W.
1879. Fowler, James Kingston, M.D., 35, Clarges-street, Mayfair, W. S 2, C.
1873. Fox, Arthur Edward Wellington, M.B., C.M., 16, Gay-street, Bath. *Councillor.*
1887. Fox, Fortescue, M.B., Strathpeffer Spa, Ross-shire.
1871. Fox, Francis, M.R.C.S., 68, Wimpole-street, W. C 3.
1885. Fox, R. Hingston, M.D., 23, Finsbury-square, E.C.
1879. Fox, Thomas Colcott, M.B., 14, Harley-street, W. *Honorary Secretary.*
1887. Frazer, Robert Fair, 185, Lavender-hill, New Wandsworth, S.W.
1868. Freer, Alfred, J.P., Stourbridge, Worcestershire.
1886. Frith, Baptist Gamble, M.B., 29, Cornwallis-gardens, Hastings.
1884. Frost, William Adams, F.R.C.S., 17, Queen Anne-street, W. C 2.

1883. Gabbett, Henry Singer, M.D., 8, Chiswick-place, Eastbourne.
1862. Gant, Frederick James, F.R.C.S., 16, Connaught-square, W. P, VP 2, LL, O, C 3.
1847. *Garrod, Sir Alfred Baring, M.D., F.R.S., 10, Harley-street, W. P, VP 2, LL, O, C 9.
1887. Garrod, Archibald Edward, M.D., 9, Chandos-street, Cavendish-square, W.
1887. Gay, John, 119, Upper Richmond-road, Putney, S.W.
1879. Gibbes, Heneage, M.D., The University, Michigan, U.S.A.
1856. Gibbon, Septimus, M.B., 39, Oxford-terrace, W.
1882. Gibbons, Robert Alexander, M.D., 29, Cadogan-place, S.W.
1881. Giffard, Douglas W., M.R.C.S., 5, Pavilion-parade, Brighton.
1867. Gill, William, M.R.C.S., 11, Russell-square, W.C.
1869. Godson, Clement, M.D., 9, Grosvenor-street, W. VP, C 3, S 2, SM.

1873. Goodsall, David Henry, F.R.C.S., 17, Devonshire-place, Portland-place, W. s 2, sm, cfc 6. *Councillor.*
1880. Goude, Herbert, M.D., Smallpox Hospital, Highgate-hill, N.
1878. *Gould, Alfred Pearce, M.S., 10, Queen Anne-street, W. s 2, c 3.
1876. Gowers, William Richard, M.D., F.R.S., 50, Queen Anne-street, W. vp, c, sm, ll.
1874. Gowland, Peter Yeames, F.R.C.S., 34, Finsbury-square, E.C.
1887. Grant, James Edward Roney, 2, Charing Cross-chambers, Duke-street, Adelphi, W.C.
1881. Green, Thomas Henry, M.D., 74, Wimpole-street, W. c 2. vp.
1868. Gregson, George, M.R.C.S., 63, Harley-street.
1886. Greves, Edwin Hyla, M.D., Rodney House, Bournemouth.
1873. Grieve, Robert, M.D., British Guiana.
1884. Griffith, David Charles Ballinger, M.R.C.P. Edin., 3, Lansdowne-place, Brighton.
1875. Griffith, G. de Gorrequer, M.R.C.S., 34, St. George's-square, S.W.
1885. Griffiths, Charles Thomas, L.R.C.P., 206, Lozells-road, Birmingham.
1884. Griffiths, Herbert Tyrrell, M.B., 5, Kensington-square, W.
1880. Gristock, William, M.D. Lond., 6, Finchley-road, N.W.

1886. Habershon, S. Herbert, M.D., 70, Brook-street, Grosvenor-square, W.
1884. Hadden, Walter Baugh, M.D., 21, Welbeck-street, Cavendish-square, W. c 2.
1891. Hadley, Wilfred J., M.B., Gloucester House, Cavendish-road, Clapham, S.W.
1887. Haig, Alexander, M.B. Lond., 7, Brook-street, W.
1884. Hairsine, Hudson, Roos, near Hull, Yorkshire.
1881. Hall, Charles Ross, M.R.C.S., Hatfield, Herts.
1874. *Hall, Francis de Havilland, M.D., 47, Wimpole-street, W. c 4, s 2, sm.
1885. Halpin, Richard Frederich Bestall, Arklow, co. Wicklow, Ireland.
1881. Hames, George Henry, F.R.C.S., 29, Hertford-street, Mayfair, W.
1879. Hamilton, Seton Guthrie, Surgeon 1st Life Guards.
1891. Handfield-Jones, Montagu, M.D., 35, Cavendish-square, W.
1887. Handford, Henry, M.D. Edin., 14, Regent-street, Nottingham.
1888. Hare, Hobart Amory, 117, South Twenty-second-street, Philadelphia. fm. 1888.
1891. Harold, John Patrick, M.R.C.S., Charing Cross Hospital, W.C.
1882. Harper, Gerald Samuel, M.B., 40, Curzon-street, Mayfair, W.
1871. Harris, Charles James, 4, Kilburn Priory, N.W.
1873. Harris, William John, Church House, Heene, Worthing.

1871. Harrison, Reginald, F.R.C.S., 6, Lower Berkeley-street, Portman-square. LL, C 1. *Vice-President.*

1883. *Hartridge, Gustavus, F.R.C.S., 65, Green-street, Grosvenor-square, W.

1864. Harvey, John Alexander, 35, Princes-square, Bayswater, W.

1882. Harvey, John Stephenson Selwyn, M.D., Durham, 1, Astwood-road, Cromwell-road, S.W.

1882. Haslam, William Frederick, F.R.C.S., 33, Paradise-street, Birmingham. C. *Councillor.*

1852. Haward, Edwin, M.D., 34A, Gloucester-place, W.

1883. Hawken, Charles St. Aubyn, 20, North-terrace, Wandsworth, S.W.

1889. Hawkins, Francis Henry, M.B., 59, Wimpole-street, W.

1890. Hebb, Frederick Theodore, M.R.C.S., 7, Milner-terrace, Cadogan-square, S.W.

1884. Hensman, Arthur, F.R.C.S., 31, Harley-street, W.

1891. Hensman, Frank, M.R.C.S., Surgeon-Major, 1st Life Guards.

1883. Herman, George Ernest, M.B., 20, Harley-street, W.

1879. Heron, George Allan, M.D., 57, Harley-street, Cavendish-square, W. *Councillor.*

1886. Herringham, Wilmot Parker, M.B., 13, Upper Wimpole-street, W.

1883. Herschell, George A., M.D., 5, West-street, Finsbury-circus, E.C.

1883. Hewitt, Frederick William, M.D., 10, George-street, Hanover-square, W.

1876. Heycock, Francis Raworth, C.M., 26, Upper Wimpole-street. W.

1872. Hicks, John Braxton, M.D., F.R.S., 34, George-street, Hanover-square. C.

1890. Hickson, Albert T., M.D., 29, Norland-square, W.

1884. Hill, Berkeley, F.R.C.S., 66, Wimpole-street, W.

1867. Hill, Thomas Harvey, M.R.C.S., 34, St. John's-street, Oxford. T, C 3.

1873. Hobson, William Henry, M.R.C.S., Great Berkhamstead, Herts.

1879. Hogg, Arthur John, M.R.C.S., Leslie Lodge, Haven-green, Ealing, W.

1884. Holland, Charles Edward, M.B., 44, Warwick-road, Maida-vale, W.

1888. Holm, John, F.R.C.S. Edin., 48, Conduit-street, W.

1868. Holman, Constantine, M.D., The Baron's, Reigate. C 4.

1881. Hood, Donald William Charles, M.D., 43, Green-street, W. *Councillor.*

1879. Hookham, Paul (address uncommunicated).

1875. Hope, William, M.D., 56, Curzon-street, Mayfair, W.

1883. *Hovell, T. Mark, F.R.C.S. Edin., 3, Mansfield-street, W.

1886. Huddart, Cuthbert Henry Cooke, M.B., 77, Redcliffe-gardens, S.W.

1885. Hudson, Charles Elliott Leopold Barton, F.R.C.S., Middlesex Hospital, W.

1890. Hughes, Edgar, F.R.C.S., 10, Old Cavendish-street, W.

1864. Hume, Frederick Henry, M.D., 53, Devonshire-street, Islington, N.

1884. Hunter, Sir William Guyer G., M.D., M.P., 21, Norfolk-crescent, Hyde Park, W.
1889. Hunter, William, M.D., 61, Wimpole-street, W.
1881. Hutchinson, Jonathan, F.R.C.S., F.R.S., 15, Cavendish-square. ll, c 4, o.
1875. Hutchinson, Samuel John, M.R.C.S., 64, Brook-street, W.

1889. I'Anson, William Andrew, Westgate Hill House, Newcastle-on-Tyne.

1884. *Jackson, Frederick William, M.D., Yorkgate House, Broadstairs.
1885. Jackson, James, M.R.C.S., 15, Huntingdon-street, Barnsbury, N.
1868. Jackson, John Hughlings, M.D., F.R.S., 3, Manchester-square, W. p, vp, o, c 5.
1853. Jackson, Robert, M.D., 53, Notting-hill-square, W.
1874. Jagielski, Victor Apollinaris, M.D., 54, York-terrace, Regent's Park, N.W.
1882. James, Joseph Brindley, M.R.C.S., 47, Jamaica-road, Bermondsey, S.E.
1887. Jamison, Arthur Andrew, M.D., 18, Lowndes-street, Belgrave-square, S.W.
1884. Jennings, Charles Egerton, F.R.C.S. (address uncommunicated).
1886. Jervis, Arthur (address uncommunicated).
1883. Jessett, Frederick Bowreman, F.R.C.S., 16, Upper Wimpole-street, W.
1883. Jessop, Walter Hamilton, F.R.C.S., 73, Harley-street, W.
1886. Johnston, James, M.D., 11, Chester-place, Hyde Park-square, W.
1888. Jones, Arthur Henry, M.D., 45, Sheep-street, Northampton.
1890. Jones, H. Macnaughton, M.D., 141, Harley-street, Cavendish-square, W.
1888. Jones, John Talfourd, M.B. Lond., Rosebank, South-terrace, Eastbourne, Sussex.
1881. Jones, Thomas William Carmalt, F.R.C.S. Edin., Abchurch House, Sherborne-lane, King William-street, E.C.
1877. Juler, Henry Edward, F.R.C.S., 77, Wimpole-street, W.

1889. Kauffmann, Otto Jackson, M.D., Seamen's Hospital, Greenwich, S.E.
1874. Kavanagh, Patrick, M.D., 186, Lewisham High-road, S.E.
1891. Keegan, Denis Francis, M.D., Surgeon-Major, The Residency, Indore, Central India.
1884. Keetley, Charles Bell, F.R.C.S., 56, Grosvenor-street, W. c. *Councillor.*
1847. *Kellock, William Berry, M.D., Stamford-hill, N.
1890. Kelly, Augustin Bernard, M.R.C.S., 82, Park-street, Grosvenor-square, W.

1891. Kelson, William Henry, M.B., 46, Watling-street, E.C.
1883. Kemp, John Robert, M.R.C.S., 101, Jermyn-street, S.W.
1890. Ker, Hugh Richard, F.R.C.S. Edin., Devonshire Cottage, Balham Hill, S.W.
1884. Kerr, Norman, M.D., 42, Grove-road, N.W.
1881. Keser, Jean Samuel, M.D., 11, Harley-street, W. c 2. *Honorary Secretary for Foreign Correspondence.*
1876. Key, Augustus Cooper, M.R.C.P. Edin., 30, Wilton-place, S.W.
1886. Kidd, Percy, M.D., 60, Brook-street, W. *Councillor.*
1889. Kirkham, Frederick William, Downham Market, Norfolk.
1883. Knapton, George, Cliveden House, 4, Cliveden-place, Eaton-square, S.W.
1875. Knox, John, M.D., Bethnal Green Infirmary, E.

1889. Lake, Richard, M.R.C.S., Thornleigh, Barnes, S.W.
1868. Lake, William Charles, M.D., Teignmouth, Devon.
1883. Langford, Phineas Pitts, M.D., Park Lodge, East Finchley, N.
1881. Langton, John, F.R.C.S., 62, Harley-street, W. c 2.
1890. Law, Edward, M.D. Edin., 35, Harley-street, W.
1890. Lawrie, Edward, M.B. Edin., Surgeon-Major, Bengal Army, The Residency, Hyderabad.
1858. Lawson, George, F.R.C.S., 12, Harley-street, W. vp 2, c 3.
1889. Lee, Henry, F.R.C.S., 9, Savile-row, W.
1887. Leggatt, Charles Ashley Scott, M.D., 2, Walton-place, S.W.
1858. Lemon, Oliver, M.R.C.S., 12, The Grange, Highbury, N.
1886. Lewers, Arthur Hamilton Nicholson, M.D. Lond., 60, Wimpole-street, W.
1867. Lichtenberg, George, M.D., 47, Finsbury-square. c 2.
1878. Lister, Sir Joseph, Bart., D.C.L., LL.D., F.R.C.S., F.R.S., 12, Park-crescent, Portland-place. o.
1890. Lithgow, Robert Alexander Douglas, M.D., 27a, Lowndes-street, S.W.
1889. Little, Ernest Muirhead, F.R.C.S., 18, Park-street, Grosvenor-square, W.
1889. Little, Fletcher, M.D., 60, Welbeck-street.
1887. Lloyd, Robert Hodgens, M.D., Brook House, Kennington-road, S.E.
1886. Lloyd, Samuel, M.D., 4, High-street, Bloomsbury, W.C.
1878. Lockwood, Charles Barrett, F.R.C.S., 19, Upper Berkeley-street, W. c.
1873. Loe, James Scarborough, 26, Woodhouse-lane, Leeds.
1881. Lorimer, G., M.D., Buxton, Derbyshire.
1868. Lowe, John, M.D., J.P., 4, Gloucester-place, Portman-square, W. c 3.
1868. *Lund, Edward, F.R.C.S., 22, St. John's-street, Manchester. o, c 3.

1889. Lunn, Henry Simpson, M.D., 5, Endsleigh-gardens, N.W.
1889. Lush, Percy, M.B., 8, Fitzjohn's-avenue, Hampstead, N.W.

1884. MacBryan, Henry Crawford, Tue Brook Villa Private Asylum, Liverpool.
1871. MacCormac, Sir William, F.R.C.S., 13, Harley-street, W. p, vp, § 2, c 4, o.
1882. MacKellar, Alexander Oberlin, F.R.C.S., 79, Wimpole-street, W.
1862. Mackenzie, Sir Morell, M.D., 19, Harley-street, W.
1880. Mackenzie, Stephen, M.D., 18, Cavendish-square, W. c 2, ll. *Vice-President.*
1881. Maclagan, Thomas John, M.D., 9, Cadogan-place, S.W. c 3.
1861. MacLaren, Alexander Connell, 60, Harley-street, W.
1891. Maclean, Allan, Harpenden Hall, Herts.
1887. Macready, Jonathan Foster Christian Horace, F.R.C.S., 51, Queen Anne-street, W.
1883. Maddick, Edmund Distin, F.R.C.S. Edin., 2, Chandos-street, Cavendish-square, W.
1885. Maguire, Robert, M.D., 4, Seymour-street, W. c 2.
1878. Mair, Robert Slater, M.D., 28, Ledbury-road, Bayswater, W.
1890. Malcolm, John David, F.R.C.S. Edin., 24, Bryanston-street, Portman-square, W.
1887. Mantle, Alfred, M.D., 12, Park-road, Halifax.
1888. Mapother, Edward Dillon, M.D., 32, Cavendish-square, W.
1891. Marsh, Howard, F.R.C.S., 30, Bruton-street, W.
1873. Marshall, Edward, M.R.C.S., Mitcham, Surrey.
1869. Marshall, William, M.D., Torrieburn, Barnes, S.W.
1864. Marshall, William Gurslave, F.R.C.S., The Asylum, Colney Hatch, N.
1889. Martin, John Michael Harding, M.D., Arnheim, Blackburn, Lancashire.
1890. Martin, Sidney, M.D., 10, Mansfield-street, W.
1884. Matheson, Farquhar, M.B., 11, Soho-square, W.
1885. McConnel, Henry Wilson, M.B., Litchdon, Barnstaple, Devon.
1884. McGann, Terence Joseph, Surgeon-Major, Madras Army, Bangalore.
1885. McGeagh, Thomas Edwin Foster, M.D., 23, New Cavendish-street, W.
1873. McHardy, Malcolm Macdonald, F.R.C.S. Edin., 5, Savile-row, W.
1884. Meredith, William Appleton, C.M., 6, Queen Anne-street, W.
1864. Middlemist, Robert Percy, M.R.C.S., 6, Devonport-street, Hyde Park, W. c 4.
1881. Millican, Kenneth William, M.R.C.S., 31, Hogarth-road, Earl's Court.
1882. Mills, Joseph, M.R.C.S., 28, Queen Anne-street, Cavendish-square, W.

1878. Milner, Edward, 32, New Cavendish-street, W., Surgeon to the Lock Hospital.
1883. Money, Angel, M.D., 24, Harley-street, W. c.
1883. Moore, Thomas, F.R.C.S., 6, Lee-terrace, Blackheath, S.E.
1883. Morgan, John Hammond, F.R.C.S., 68, Grosvenor-street, W. s 2.
1871. Morley, Alexander, 42, Albemarle-street, W.
1881. Morris, Henry, F.R.C.S., 8, Cavendish-square, W. c.
1878. Morris, Malcolm Alexander, F.R.C.S. Edin., 8, Harley-street, W. c 2.
1882. Morton, Andrew Stanford, F.R.C.S., 26, Weymouth-street, Portland-place, W.
1884. *Moullin, Charles William Mansell, F.R.C.S., 69, Wimpole-street, W.
1883. Mowat, George, M.R.C.P. Edin., St. Albans.
1878. Mumford, William Lugar, M.D., 12, Suffolk-street, Pall-mall, S.W.
1886. Murphy, James, M.D., Holly House, Sunderland.
1884. Murray, Fred., M.B., Durbanville, Cape Colony, South Africa.
1890. Murray, George, M.R.C.S., 34, Wimpole-street, Cavendish-square, W.
1886. Murray, Hubert Montague, M.D., 27, Savile-row, W.
1883. Murray, James, M.D., 21, Weymouth-street, W.
1879. Murrell, William, M.D., 38, Weymouth-street, W.
1885. Myers, Arthur Thomas, M.D., 9, Lower Berkeley-street, W.
1873. Myers, Henry Reynolds, 30, Euston-square, N.W.

1877. Nesbitt, Dawson, M.D., 1, Norfolk-square, Hyde Park, W.
1876. Newham, James, 80, Gloucester-place, W.
1889. *Nias, J. Baldwin, M.B., 40, Brook-street, Grosvenor-square, W.
1880. Nix, Edward James, M.D., 143, Great Portland-street, W.

1887. Oakley, Adam Robert Hamilton, L.R.C.P., 2, Bennet-street, St. James's, S.W.
1885. Ogilvie, Leslie, M.B., 46, Welbeck-street, W.
1884. Ogle, Charles John, 36, Great Marlborough-street, W.
1884. Oliver, George, M.D., West End Park, Harrogate.
1875. Ord, William Miller, M.D., 37, Upper Brook-street, W. p, c 4.
1887. Ormerod, Joseph Arderne, M.D., 25, Upper Wimpole-street, W.
1889. Orton, George Hunt, M.B., 1a, Campden Hill-road, Kensington, W.
1884. Orwin, Arthur Wigelsworth, M.D., 15, Weymouth-street, Portland-place, W.
1880. Oswald, James Waddell Jeffreys, M.D., 245, Kennington-road, S.E.
1883. Owen, Charles J. Rayley, 14, Devonshire-terrace, W.
1878. *Owen, Edmund, F.R.C.S., 64, Great Cumberland-place, W. vp 2, c 3, s 2, sm, ll. *Trustee.*
1881. Owen, Isambard, M.D., 40, Curzon-street, Mayfair, W. s 2, c 3. *Councillor.*

1886. Paget, Stephen, F.R.C.S., 57, Wimpole-street, W. *Councillor.*
1880. Palmer, Frederick Stephen, M.D. Durham, Compton Lodge, East Sheen, S.W.
1882. Palmer, William Pitt, M.B. Durham, 17, Belgrave-square, Torquay.
1877. *Paramore, Richard, M.D., 2, Gordon-square, W.C.
1867. Parkinson, George, 50, Brook-street, Grosvenor-square, W.
1880. Parkinson, George William, 1, Bentinck-street, Cavendish-square, W.
1881. Parrott, Edward John, M.R.C.S., Hayes, Uxbridge, Middlesex.
1871. Parsons, Francis Henry, M.D., "The Hurst," West Worthing.
1885. Pasteur, William, M.D., 4, Chandos-street, Cavendish-square, W.
1872. Patten, Charles Arthur, M.R.C.S., Marpool House, Ealing, W.
1890. Pattison, Edward Seton, M.R.C.S., Granville House, Fulham-park, S.W.
1861. Paul, John Hayball, M.D., Camberwell House, Camberwell, S.E. c 6.
1854. Pavy, Frederick William, M.D., F.R.S., 35, Grosvenor-street, W. vp, ll, c.
1881. *Peacey, William, M.B., 214, Lewisham High-road, S.E.
1883. Peck, Edward George, M.A., Queensbury, Bradford, Yorks.
1871. Pedler, George Henry, M.R.C.S., 6, Trevor-terrace, Knightsbridge, S.W.
1883. Perigal, Arthur, M.D., New Barnet.
1876. Phillips, Charles Douglas Ferguson, M.D., F.R.S.E., 10, Henrietta-street, Cavendish-square, W. c 3.
1873. Phillips, George Richard Turner, M.R.C.S., 24, Leinster-square, Hyde Park, W. c 2.
1885. Phillips, John, M.D., 71, Grosvenor-street, W.
1883. Phillips, Sidney Philip, M.D. Lond., 61, Upper Berkeley-street, Portman-square, W.
1878. Phillips, Sutherland Rees, M.D., St. Ann's Heath, Virginia Water, Berks.
1883. Pick, Thomas Pickering, F.R.C.S., 18, Portman-street, W. c 2.
1884. Piesse, C. H., M.R.C.S., 2, New Bond-street, W.
1883. Pitts, Bernard, F.R.C.S., 31, Harley-street, Cavendish-square. c 4, s 2. *Councillor.*
1886. Pocock, Walter, Gwydyr House, 58, Brixton-hill, S.W.
1890. Pope, Harry Campbell, M.D. Lond., 280, Goldhawk-road, Shepherd's Bush, W.
1873. Port, Heinrich, M.D., 48, Finsbury-square, E.C.
1850. *Potts, William, F.R.C.S., 2, Albert-terrace, Regent's Park, N.W. c 3.
1868. Powell, Josiah Taylor, M.D., 347, City-road, E.C.
1871. Powell, Richard Douglas, M.D., 62, Wimpole-street, W. vp, c 4, o. *President.*
1891. *Prickett, Marmaduke, M.D., 12, Devonport-street, Gloucester-square, W.
1885. Pringle, John James, M.B., 35, Bruton-street, Berkeley-square, W.

1889. Pritchard, Owen, M.D., 37, Southwick-street, Hyde Park, W.
1873. Purcell, Ferdinand Albert, M.D., 7, Manchester-square, W.
1878. Pye, Walter, F.R.C.S., 4, Sackville-street, Piccadilly, W. c 2.

1870. Quain, Sir Richard, Bart., M.D., F.R.S., 67, Harley-street, W. vp, c 3.

1883. Ralfe, Charles Henry, M.D., 26, Queen Anne-street, W.
1861. Ramskill, Jabez Spence, M.D., 5, St. Helen's-place, E.C.
1881. Ranking, John Ebenezer, M.D., Hanover House, Tunbridge Wells.
1859. *Rayner, John, M.R.C.P. Edin., Swaledale House, Highbury-quadrant, N.
1890. Rayner, William, M.R.C.S., 4, Dorset-square, N.W.
1850. *Read, Reginald, F.R.C.P. Edin., 15 Windsor-road, Denmark-hill, S.E.
1879. Reeves, Henry Albert, F.R.C.S. Edin., 6, Grosvenor-street, W.
1890. Reid, John, M.B., 1, Bedford-gardens, Clapham, S.W.
1882. Reid, Thomas Whitehead, F.R.C.P. Edin., 34, St. George's-place, Canterbury.
1887. Remfry, Leonard, M.D., 4, Harley-street, W.
1872. Reynolds, John Russell, M.D., F.R.S., 38, Grosvenor-street, W. c 3.
1872. Richards, Joseph Peeke, M.R.C.S., County Asylum, Hanwell, W. c. 2.
1850. *Richardson, Benjamin Ward, M.D., LL.D., F.R.S., 25, Manchester-square, W. p, vp, ll, c 5, o, fm 1854.
1830. *Robarts, Henry Pratt, F.R.C.S., 31, Great Coram-street, W.C. vp 2, s 9, c 10, fm 1844, sm.
1891. Robb, James Taylor, M.D., 33, Lowndes-street, Belgrave-square, W.
1868. *Roberts, David Lloyd, M.D., F.R.S.E., 11, St. John's-street, Manchester.
1857. Roberts, David Watkin, M.D., 56, Manchester-street, W.
1885. Roberts, Edward Coleridge, M.R.C.S., Southgate, N.
1874. Roberts, Frederick Thomas, M.D., 102, Harley-street, W.
1889. Roberts, Sir William, M.D., F.R.S., 8, Manchester-square, W. c.
1873. Robertson, William Henry, M.D., J.P., 6, The Square, Buxton, Derbyshire.
1884. Robinson, Arthur Henry, M.D., The Infirmary, Bancroft-road, N.E.
1847. *Rogers, William Richard, M.D., 56, Berners-street, W. vp, c 6.
1890. Root, Arthur Guernsey, M.D., Albany, New York, U.S.A.
1886. Rose, Robert Duncan, F.R.C.S., St. Leonard's-place, York.
1874. Rose, William, F.R.C.S., 17, Harley-street. c 2. *Vice-President, Lettsomian Lecturer.*
1883. *Ross, Daniel McClure, F.R.C.S., 54, Upper Berkeley-street, W.
1888. Roth, Bernard, F.R.C.S., 29, Queen Anne-street, W.
1876. Routh, Alfred Curtis, 33, Marina, St. Leonards-on-Sea.
1881. Routh, Amand, M.D., 14a, Manchester-square, W. c 2.
1848. *Routh, Charles Henry Felix, M.D., 52, Montagu-square, W. p, vp 2, o, ll, s 4, c 6, sm. *Trustee.*

1888. Rugg, George Lewis, M.R.C.S., Ivy Bank, Crown Hill, Lower Norwood.

1887. Rushworth, Frank, M.D., "Langdale," Goldhurst-terrace, South Hampstead, N.W.

1889. Russell, James Samuel Risien, M.B. *Travelling.*

1886. Rutherfoord, Henry Trotter, M.D., 46, Queen Anne-street, W.

1879. Ryley, James Beresford, M.D., 1, Bentinck-street, W.

1887. Sainsbury, Harrington, M.D., 63, Welbeck-street, W.

1884. Salter, Thomas Knight, M.R.C.S., 23, Lower Seymour-street, W.

1863. *Sansom, Arthur Ernest, M.D., 84, Harley-street, W. vp, s 2, c 5, sm, §, ll, o.

1886. Savage, George Henry, M.D., 3, Henrietta-street, W. c.

1886. Savill, Thomas Dixon, M.D., Paddington Infirmary, W.

1873. Scott, William, M.D., Waverley House, Huddersfield.

1873. Sedgwick, James, M.D., Boroughbridge, Yorkshire.

1868. Sedgwick, Leonard William, M.D., 2, Gloucester-terrace, Hyde Park, W. vp 2, c 4, § 3.

1883. Semon, Felix, M.D., 39, Wimpole-street, W. §, c.

1887. Servais, Leopold, M.D., Antwerp, Belgium.

1876. Sewell, Charles Brodie, M.D., 21, Cavendish-square, W. c.

1889. Shaw, George, M.B. Lond., 46, Western-road, Hove.

1884. Shaw, John, M.D., Burlington House, Willoughby-road, Hampstead, N.W.

1886. Sheild, Arthur Marmaduke, F.R.C.S., 20, Stratford-place, Oxford-street, W. c. *Honorary Secretary.*

1890. Sheppard, William John, M.D., Laurel House, Putney, S.W.

1881. Shipton, Arthur, F.R.C.S. Edin., Buxton, Derbyshire.

1878. Shipton, William Parker, M.R.C.S., J.P., Buxton, Derbyshire.

1885. Shoemaker, John V., M.D., 1031, Walnut-street, Philadelphia, U.S.A.

1890. Silk, John Frederick William, M.D. Lond., 6, Chandos-street, Cavendish-square, W.

1890. Simon, Robert M., M.D., 27, Newhall-street, Birmingham.

1884. Simpson, James Herbert, M.D., The Crescent, Rugby, Warwickshire.

1884. Sinclair, John, M.R.C.P., General Post Office, St. Martin's-le-Grand, E.C.

1883. *Skerritt, Edward Markham, M.D., Richmond Hill, Clifton.

1886. Slater, Charles, M.B., 16, Northwick-terrace, St. John's Wood, N.W.

1862. Slight, George, M.D., 14, Old Burlington-street, W. c 2.

1889. Smale, Morton, M.R.C.S., 22a, Cavendish-square, W.

1845. *Smiles, William, M.D., St. Martha's Lodge, Guildford. vp 2, s 4, c 9, sm.

1887. Smith, Frederick John, M.D. Oxon., 4, Christopher-street, Finsbury-square, E.C.

1848. *Smith, Henry, F.R.C.S., 7, Wimpole-street, W. p, vp, ll, o, c 3.

1882. Smith, Herbert, Urmson, Oudtshorne, Cape of Good Hope, South Africa.
1873. Smith, Heywood, M.D., 18, Harley-street, W. c 3.
1880. Smith, Noble, F.R.C.S. Edin., 24, Queen Anne-street, W.
1877. Smith, Sydney Lloyd, M.R.C.S., 25, Argyle-square, King's Cross, W.C.
1882. Smith, Thomas Frederick Hugh, F.R.C.S., Farningham, Kent.
1873. *Smith, Thomas Gilbart, M.D., 68, Harley-street, W. vp 2, s 2, sm, c 4. *Trustee.*
1872. Smith, Walter, M.R.C.P. Edin., 2, Stanhope-terrace, Gloucester-gate, Regent's Park, N.W.
1874. Smyth, William Woods, Maidstone.
1888. Spencer, John, Lyons-terrace, Hetton, Durham.
1869. Spender, John Kent, M.D., 17, Circus, Bath. fm 1874.
1887. Spicer, Scanes, M.D., 28, Welbeck-street, W.
1883. Spitta, Edmund Johnson, M.R.C.S., Ivy House, Clapham Common, S.W.
1864. Squire, Alexander John Balmanno, M.B., 24, Weymouth-street, Portland-place.
1881. Startin, James, M.R.C.S., 17, Sackville-street, W.
1884. Stephens, William John, 41, Grand-parade, Brighton.
1882. Stewart, James, M.R.C.P. Edin., Dunmurry, Sneyd-park, Clifton.
1891. Stewart, William Edward, F.R.C.S. Edin., 16, Harley-street, W.
1883. Stewart, William Robert Henry, F.R.C.S. Edin., 41, Devonshire-street, Portland-place, W.
1884. Stiven, Edward Winnan Fleming, M.D., Lincoln House, Harrow, Middlesex.
1885. Stivens, B. H. Lyne, M.D., 11, Kensington Gardens-square, W.
1848. *Stocker, John Sherwood, M.D., 2, Montagu-square, W. c 10, s 2.
1884. Stoker, George, 14, Hertford-street, Mayfair, W.
1877. Stowers, James Herbert, M.D., 41, Finsbury-square, E.C.
1873. Strange, William Heath, M.D., 5, Grosvenor-street, W. c 3.
1881. Sturge, William Allen, M.D., 9, Rue Longchamp, Nice. sm.
1889. Sumpter, Walter John Ernely, M.R.C.S., Sherringham, Norfolk.
1876. *Sutherland, Henry, M.D., 6, Richmond-terrace, Whitehall, S.W.
1880. Sweeting, Richard Deane Roker, M.R.C.S., 39, Addison-gardens, W.
1885. *Syers, Henry Walter, M.D. Camb., 3, Devonshire-street, Portland-place, W.
1881. Sykes, Edwin John, M.B., York House, Tottenham.
1884. Symonds, Horatio Percy, F.R.C.S., 35, Beaumont-street, Oxford.

1885. Tadlock, A. B., M.D., Knox Ville, Tennessee, U.S.A.
1864. Tait, Edward Wilmshurst, 48, Highbury-park, N.
1879. *Tait, Lawson, F.R.C.S., 7, The Crescent, Birmingham.
1875. Tamplin, Charles Harris, "Lindfield," Crescent-road, Ramsgate.
1882. Taylor, Seymour, M.D., 16, Seymour-street, Portman-square, W. c 2.

1884. Taylor, Sydney Hamilton, M.D., 1, Park-avenue, Willesden-park, N.W.
1859. Thompson, Edmund Symes, M.D., 33, Cavendish-square, W. vp, o, s 3, c 3, sm.
1855. Thompson, Sir Henry, F.R.C.S., 35, Wimpole-street, W.
1873. Thomson, John Roberts, M.D., Monkchester, Bournemouth, Hants.
1884. Thomson, William Sinclair, M.D., 1, Palace-court, Notting-hill-gate, W.
1876. Thornton, John Knowsley, M.C., 22, Portman-street, W. p, vp, c 2. *Councillor.*
1867. Thorowgood, John Charles, M.D., 61, Welbeck-street, W. ll, s 2, sm, c 3.
1856. Thudichum, John William Louis, M.D., 11, Pembroke-gardens, Kensington, W. vp, ll, o, c.
1884. Thursfield, Thomas William, M.D., J.P., Selwood, Beauchamp-square, Leamington.
1876. Tibbits, Herbert, M.D., 68, Wimpole-street, W.
1867. Timms, Godwin William, M.D., 9, Wimpole-street, W.
1865. Travers, William, M.D., 2, Phillimore-gardens, Kensington.
1884. *Treves, Frederick, F.R.C.S., 6, Wimpole-street, W. c 2.
1882. Tuke, Charles Molesworth, Manor House, Chiswick.
1886. Tuke, Thomas Seymour, M.B. Oxon., Manor House, Chiswick.
1884. Turner, G. R., F.R.C.S., 49, Green-street, Grosvenor-square, W.
1890. Tweed, Edward Reginald, M.D., 55, Upper Brook-street, W.
1883. Tweedy, John, F.R.C.S., 100, Harley-street, W.
1891. Tyson, William Joseph, M.D. Durham, 10, Langhorne-gardens, Folkestone.

1887. *Underwood, Edward T., M.D., Fort Bombay, India.

1878. Vasey, Charles, 112, Cambridge-gardens, W.
1886. Veale, Henry Richard Lord, M.D. (Dep. Surgeon-General), 119, St. George's-road, S.W.
1883. Venning, Edgcombe, F.R.C.S., 30, Cadogan-place, S.W.
1874. Verley, Reginald Louis, F.R.C.P. Edin., 28b, Devonshire-street, Portland-place, W.

1850. *Waggett, John, M.D., Perivale, Bournemouth; and Union Club, S.W.
1889. Wakefield, Thomas, M.B., 37, Nottingham-place, W.
1884. Wakley, Thomas, 5, Queen's-gate, S.W.
1850. *Wakley, Thomas Henry, F.R.C.S., 5, Queen's-gate, S.W.
1880. Walsham, William Johnson, F.R.C.S., 27, Weymouth-street, W. c.
1881. Warner, Francis, M.D., 5, Prince of Wales-terrace, W.
1883. Waterhouse, William Dakin, LL.D., 18, Woodchurch-road, West Hampstead, N.W.

1872. Waters, John, M.R.C.S., 41, Bloomsbury-square, W.C.
1868. Watkins, Charles Stuart, M.R.C.S., 16, King William-street, Strand, W.C.
1891. Watson, W. Spencer, F.R.C.S., 7, Henrietta-street, Cavendish-square, W.
1889. Waugh, Henry Dunn, M.D., 6, Sumner-place, Onslow-square, S.W.
1884. Webb, F. Ernest, M.R.C.S., 113, Maida-vale, W.
1889. Weber, Hermann, M.D., 10, Grosvenor-street, W.
1887. Webster, Henry William, M.D., St. George's Infirmary, Fulham-road, S.W.
1882. Wells, Charles, M.D., 69, Finchley New-road, N.W.
1838. *Wells, John Robinson, F.R.C.S., 4, Pierrepoint-road, Acton, W. c 2.
1884. West, Samuel, M.D., 15, Wimpole-street, W. s 2, c 2. *Chairman of House and Finance Committee.*
1889. Wethered, Frank Joseph, M.D., 34, Queen Anne-street, W.
1882. Whipham, Thomas T., M.D., 11, Grosvenor-street, W. sm, c.
1884. Whistler, William MacNeill, M.D., 17, Wimpole-street, W.
1889. White, E. F., F.R.C.S., 7, Dealtry-road, Putney, S.W.
1868. White, Joseph, F.R.C.S. Edin., Oxford-street, Nottingham.
1880. *White, William Henry, M.D., 43, Weymouth-street, W. c. 3.
1885. White-Cooper, George Owen, M.B., 5, Cranley-gardens, S.W.
1883. Whitehead, Walter, F.R.C.S. Edin., F.R.S.E., 499, Oxford-road, Manchester.
1885. Whitla, William, M.D., 8, College-square North, Belfast, Ireland.
1877. Whitmore, William Tickle, 7, Arlington-street, Piccadilly, W.
1862. Willett, Edmund Sparshall, M.D., Wyke House, Isleworth. c 3.
1872. Williams, Charles Theodore, M.D., 2, Upper Brook-street, Grosvenor-square, W. p, vp 2, ll, s 2, sm 2, o 1, l 3, c 9.
1876. Williams, Henry William, M.D., 7, Chapel-place, Cavendish-square, W.
1883. Williams, John, M.D., 63, Brook-street. c 3.
1883. Willis, Arthur Keith, M.A. Oxon., Gascony House, West End-lane, N.W.
1881. Wills, Caleb Shera, C.B., Brigade Surgeon, Army Medical Department, Loyterton, Beckenham, Kent.
1873. Wills, Thomas Munns, F.R.C.S.I., J.P., 44, Merton-road, Bootle, Liverpool.
1884. Winslow, H. Forbes, M.D., 14, York-place, Portman-square, W.
1873. Winslow, Lyttleton Stewart Forbes, D.C.L., M.B., 70, Wimpole-street, W. c.
1876. Woakes, Edward, M.D., 78, Harley-street, W.
1882. Wolfenden, Richard Norris, M.D., 35, Harley-street, W.
1886. Wood, T. Outterson, M.D., 40, Margaret-street, Cavendish-square, W. c 3.
1873. Woodhouse, Robert Hall, M.R.C.S., 1, Hanover-square, W.
1889. Woolfson, de, Louis E. G., 26, St. John's-hill, Shrewsbury.

1891. Woollett, Charles Jerome, M.R.C.S., 35, Telfourd-avenue, Streatham-hill, S.W.
1886. Wordsworth, William John, 4, Tisbury-road, West Brighton.
1884. Wyman, William Sanderson, M.D., Red Brae, Putney-hill, S.W. c 2.

1891. Yarr, Michael Thomas, Surgeon, Medical Staff, Junior Army and Navy Club, St. James's-street, S.W.
1884. Yeo, I. Burney, M.D., 44, Hertford-street, Mayfair, W.
1884. Younger, Edward George, M.D., 26, St. James's-street, Bedford-row, W.C.

NON-SUBSCRIBING FELLOWS.

1868. Aitken, Sir William, M.D., F.R.S., Woolston, Southampton.
1871. Arnold, Wilberforce, J.P., Crescent House, Belfast.
1868. Bateman, Frederic, M.D., J.P., Upper-street, Giles-street, Norwich.
1868. Beatty, Thomas Carlyle, Seaham Harbour, Durham.
1872. Bell, John Hougham, M.D., Downside, Ventnor, Isle of Wight.
1868. Braidwood, Peter Murray, M.D., 11, Grosvenor-gardens, Willesden Green, N.W. fm 1877.
1868. Buckle, Fleetwood, M.D., Staff Surgeon R.N.
1850. Burnie, William, M.D., Houghton House, Bradford.
1868. Child, Edwin, New Malden, Surrey.
1870. Clouston, Thomas Smith, M.D., Royal Asylum, Morningside, Edinburgh. fm 1870.
1873. Duranty, E. Nicholas, Marseilles.
1868. Fletcher, Thomas Bell Elcock, M.D., J.P., 43, Clarendon-square, Leamington.
1868. Folker, William Henry, F.R.C.S., Hanley, Staffordshire.
1869. Foster, Sir Walter B., M.D., 14, Temple-row, Birmingham.
1868. Fox, Charles Henry, M.D., Brislington House, near Bristol.
1868. Fox, John Makinson, The Grove, Lymm, Cheshire.
1868. Gaine, Charles, 30, Gay-street, Bath.
1871. Glynn, Thomas Robinson, M.D., 62, Rodney-street, Liverpool.
1872. Harris, Henry, LL.D., M.D., Redruth, Cornwall.
1868. Kirkman, William Phillips, M.D., Fearon-road, Hastings.
1868. Knaggs, Samuel, Ebor Mount, Huddersfield.
1869. Lees, Charles Alexander, M.D., Fleet Surgeon R.N.
1869. Lipscomb, John Thomas Nicholson, M.D., St. Albans, Herts.

1869. Lunn, William Joseph, M.D., Hull.
1859. Marshall, James, M.D., 6, Rubislaw-place, Aberdeen.
1869. Mathews, Robert, Bickley, Kent.
1871. Maurice, Oliver Calley, Reading.
1862. Maybury, Augustus Kingston, M.D., Holly Lodge, Richmond, Surrey.
1868. McIntyre, John, M.D., Odiham, Hants.
1868. Nevins, John Birkbeck, M.D., 3, Abercromby-square, Liverpool.
1871. Ogle, William, M.D., Derby.
1869. Pemberton, Oliver, F.R.C.S., J.P., 65, Temple-row, Birmingham.
1869. Philipson, George Hare, D.C.L., M.D., J.P., 7, Eldon-square, Newcastle-on-Tyne.
1869. Price, William Preston, M.D., 1, Ethelbert-crescent, Margate.
1869. Prior, Charles Edward, M.D., St. Peter's, Bedford.
1869. Roberts, Bransby, M.D., Badlesmere House, Eastbourne.
1871. Shettle, Richard Charles, M.D., 73, London-street, Reading.
1871. Sloman, Samuel George, Farnham.
1869. Stear, Henry, Saffron Walden, Essex.
1869. Stedman, James Remington, M.D., J.P., Guildford, Surrey.
1869. Taylor, Charles Bell, M.D., Nottingham.
1869. Webster, Frederick Richard, St. Albans, Herts.
1868. Wiblin, John, F.R.C.S., Southampton.

*** As it is very desirable that the List of Members should be kept as accurately as possible, Fellows are requested to send notice of any corrections that may be necessary to the Secretaries or to the Registrar.

GENERAL MEETING.

March 2nd, 1891.

J. Knowsley Thornton, M.C., President, in the Chair.

Annual Report of the Council, presented at the General Meeting of the Society, March 2nd, 1891.

In presenting its Annual Report, the Council has the pleasure of congratulating the Society upon another year of continued progress and prosperity.

The attendance at the Ordinary Meetings has been well maintained during the session; the papers read and the discussions thereon have been of great interest and importance, whilst the number and nature of the cases brought forward on Clinical Evenings prove that this feature of the Society's work is thoroughly appreciated by the Fellows.

The number of Fellows at present on the roll is 744, comprising:—37 Honorary, 48 Corresponding, 58 Life (including 19 of over 40 years' standing, who are exempt from the payment of further subscriptions), 42 Non-subscribing, and 559 Subscribing.

Thirty-three Subscribing Fellows have been elected during the session, and two Fellows who had previously resigned have been re-admitted at their own request.

On the other hand, however, the Council regrets to have to record no less than thirteen deaths since the issue of the last Report, viz., that of one Honorary Fellow, Dr. Henry Bigelow, of Boston, U.S.A., and of twelve Subscribing Fellows, Charles Henry Rogers-Harrison, F.R.C.S., who had served as Vice-President, Treasurer, Secretary, Councillor, and Trustee; James Matthews Duncan, M.D., F.R.S.; B. Arcedeckne Duncan, M.D.; Edward Bellamy, F.R.C.S.; George Gulliver, M.B., F.R.C.P.; Frederick Simms, M.B., late Councillor; Charles Collambell, F.R.C.S., J.P.; Edward Sandwell, M.R.C.S.; Walter Pearce, M.D.; Edmund Cuthbert Ring, M.R.C.S.; John Armstrong, M.D., Dartford; and Robert Gee, M.D., Liverpool.

Five Fellows have resigned.

In accordance with the intimation conveyed in the last Report, the Society's Thirteenth Volume was issued under the title of "Transactions" instead of "Proceedings," as heretofore, and included the Lettsomian

Lectures by Mr. Edmund Owen and the Oration by Dr. A. Ernest Sansom. These additions, together with the very full reports of the Ordinary Meetings which the volume contains, have added materially to the cost of publication, but have enabled the Council to place in the hands of each of the Fellows a complete record of the year's work.

The gift presented by the late President, Dr. C. Theodore Williams, has been expended in procuring a new oak book-case and President's desk, and Dr. C. D. F. Phillips's gift of a new Presidential chair in oak, completes a handsome and much needed addition to the furniture of the Meeting Room.

The Electric Light has been installed with the most satisfactory results, as regard comfort, lighting, and economy, in the Meeting Room, Library, and Entrance Hall, the walls and ceilings of which have also been cleaned and distempered, whilst the removal of two book-cases from the Library has considerably improved its appearance and enlarged the available floor space.

The extraordinary expenditure of the year has, therefore, been exceptionally heavy, and the Council was unable to recommend the redemption of more than one Debenture Bond.

No Essay was received for the Fothergillian Medal, and as there has been a similar absence of competitors on many previous occasions, the Council has applied to the Charity Commission for a new Scheme to govern its future award. Under this Scheme it is proposed :—

> "That instead of a Gold Medal of the value of Twenty Guineas, being given annually for the best Essay on a previously-announced subject, a Medal and Cash Premium, of the joint value of Sixty Guineas, shall be given triennially, not necessarily for an Essay on a previously-announced subject, but for work published within five years prior to the date of the award."

This Scheme* has been virtually accepted by the Commissioners and the first award under the new regulations will be made in 1893. With the view also of avoiding the legal expenses hitherto incurred whenever a change of Trustees took place, the Council, with the approval of the existing Trustees, has made arrangements for the transfer of the Fund to the custody of "The Official Trustees of Charitable Funds."

The Council is glad to take this opportunity of placing on record its indebtedness to Dr. Routh, Dr. Cogswell, and the late Mr. Rogers-Harrison for their long and valued services as Fothergillian Trustees.

The Council further is pleased to be able to announce that Dr. Gilbart Smith and Mr. Edmund Owen have kindly consented to act as Trustees, with Dr. Routh, of the Personal Property of the Society, and that Dr. Beevor has been associated with Dr. Hare and Mr. Bryant as a Trustee of the Real Estate.

To the great regret of all, Mr. Goodsall has resigned the position of Chairman of the House and Finance Committee which he had occupied for many years with so much advantage to the Society, and the Society has been very fortunate in obtaining the services of Dr. Samuel West for that important position.

The Council has to announce with deep regret that, owing to Mr. Poole's increasing infirmities, his faithful services—extending over a period of

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* *Vide* p. xi.

twenty-five years—will no longer be at the disposal of the Society after September next, and that it has made arrangements whereby he will be retired with a substantial pension ; and Mr. Hall, who has already so ably filled the post of Assistant Registrar will become the Registrar of the Society.

In conclusion, the Council has again to acknowledge its indebtedness to the Treasurer, Mr. Durham, and to the Honorary Librarian, Dr. Allchin, for their continued labours in the interests of the Society.

Report of the Honorary Librarian.

I have the honour to report that during the past year 54 volumes have been presented to the Library by Authors and Publishers, 56 by the late Mr. Rogers-Harrison ; 31 books have been purchased by the Fothergillian Fund, and 2 have been received from the New Sydenham Society, making a total of 143.

The Library continues to be increasingly used by the Fellows of the Society, more especially as regards the loan of books.

The works of the 15th, 16th, and 17th centuries have been arranged in the cases in the meeting room, in chronological order, preliminary to cataloguing, the best method of which is now engaging my attention. Progress has been made towards the repairing of these earlier works.

The removal of the Library of the Royal Historical Society has liberated much wanted space, which I propose to devote to Journals and other periodical literature, and I am pleased to be able to report that sufficient space is now at my disposal to accommodate all volumes likely to be received during the next few years.

The proposed rearrangement of the Fothergillian bequest is likely to affect the Library unfavourably as regards funds, and as hitherto it has been the sole source available for the purchase of books, it will become a matter for the Council seriously to consider how far they may be able to set aside a definite annual sum for the maintenance of the Library. At present the contributions from the Society's funds are limited to the purchase of a few journals and to binding.

The Library Committee has met from time to time to sanction the purchase of books, and consider other matters connected with the Library.

I am unwilling to allow Mr. Poole to retire from his office of Librarian without expressing the deep obligation to which the Society is under to him for the care and interest that he has always shown in this department of the Society's affairs, and also to add my own personal thanks for much assistance most willingly rendered during the past eight years. Mr. Hall, upon whom much of the active work of the Librarian has fallen during the past year, has afforded me most valuable aid in the arrangement and cataloguing of the books.

W. H. Allchin,
Hon. Librarian.

Dr. THE MEDICAL SOCIETY OF LONDON.—BALANCE SHEET, 1890–91. Cr.

1890	Receipts.	£	s.	d.	£	s.	d.
Feb. 1	To Balance from last Account				136	16	9
	,, Subscriptions (including Arrears)				475	6	10
	,, Entrance Fees				33	12	0
	,, Rents				770	16	11
	,, Contributions for use of Rooms				25	14	6
	,, Donations:						
	Dr. C. Theodore Williams for President's table, &c.	100	0	0			
	Dr. C D. F. Phillips for President's chair	10	0	0			
	J. Knowsley Thornton, Esq., for decorations at conversazione	15	0	0			
					125	0	0
					£1567	7	0

1890	Expenditure.	£	s.	d.
	By Rent, Gas and Electric Light, Coals, Rates, Taxes, and Insurance	468	14	8
	,, Repairs, including cleaning and retouching interior of premises	97	6	4
	,, Furniture in Meeting Room (*vide* "Donations")	156	11	0
	,, Library Expenses	25	19	7
	,, Salary and Collector's poundage, &c. (Registrar)	84	2	5
	,, Salary and Gratuity, Assistant Registrar	110	10	0
	,, Salary, Reporter	24	3	0
	,, Printing and Stationery	9	19	6
	,, Printing Transactions, Vol. XIII	154	15	7
	,, Postage, including issue of Vol. XIII	22	6	2
	,, Redemption of Debenture Bond	100	0	0
	,, Interest on Debenture Bonds	106	1	1
	,, Wages	48	18	0
	,, Miscellaneous Expenses, including Refreshments at Meetings, Chandlery, and Cleaning, &c.	34	10	7
	,, Conversazione (*vide* "Donations")	58	19	4
1891 Jan. 31	,, Balance at Bankers	64	9	9
		£1567	7	0

(Signed) ARTHUR E. DURHAM, *Treasurer.*

February 13th, 1891. Audited, compared with the vouchers, and found correct,

(Signed) HUGH R. BEEVOR, STEPHEN PAGET, } *Auditors.*

TRANSACTIONS

OF THE

MEDICAL SOCIETY OF LONDON.

118TH SESSION.

October 20th, 1890.

OPENING ADDRESS—ABDOMINAL SURGERY PAST AND PRESENT.

By the President, JOHN KNOWSLEY THORNTON, M.C.

FELLOWS OF THE MEDICAL SOCIETY OF LONDON,—It is, as you are aware, the custom of our ancient Society, that your President shall open the winter session by an address, and it seemed to me, on thinking over the possible subjects for my address this evening, that I could not do better, than speak upon that branch of surgery, to the study of which the greater part of my professional life has been devoted. I propose then, first, to review briefly the past history of abdominal surgery, pointing out, as I recall the work of the past, the causes of its failures, which our riper experience has taught us how, in a great measure at any rate, to avoid, and then endeavour to take stock of our present work, and indicate, to the best of my ability, the directions in which I think advances may still be made, in what I shall venture to speak of as the triumphal march of abdominal surgery.

The operation of ovariotomy must undoubtedly be regarded as the starting point of abdominal surgery, and Dr. Ephraim McDowell, of Danville, Kentucky, as the father of this great operation, and there is every reason to suppose that to his studies under Mr. John Bell, in my own Alma Mater, Edinburgh, he owed the suggestion, which he successfully put into practice in 1809.

McDowell, from his own account of the operation, appears not to have used the median incision, but to have made one nine inches long, parallel to the fibres of the rectus on the left side; the pedicle was secured by a ligature, merely tied round it, and the long ends were brought out at the lower angle of the wound, which was closed by interrupted sutures, fortified by strips of adhesive plaister, applied between every two sutures. Five days later, when he visited his patient, he found her making up her bed, and in twenty-five days she returned to her own home.

From his description of his third case, I infer that the ligature was of whipcord, and in this case he made his incision in the linea alba. In the first case he turned the patient on her side to allow the blood to escape from the peritoneum, and in his second case he appears to have used some other means for cleaning out the cavity; but his reports are very brief, and it is impossible to make out from them the exact details of his operations. It is nearly eighty years since this first great step in abdominal surgery was taken, and for fully sixty years, *i.e.*, down to 1870, or later, but little real advance was made. Ovariotomy became gradually more general, and its statistics slowly—very slowly—improved, and isolated attacks were made upon fibroid tumours of the uterus, enlarged spleens, and other abdominal tumours; but success was the exception rather than the rule, and it seemed as if ovariotomy was to hold the field alone, and with a very heavy mortality. I will now, by an examination of the methods employed during these sixty years, endeavour to show why this stagnation took place.

Dr. Nathan Smith, Professor of Surgery in Yale College, Connecticut, must also be credited as one of the originators of ovariotomy, because he operated at Norwich, Vermont, in July, 1821, with no previous knowledge of McDowell's work, and his method was nearly perfect, for his incision was only three inches; he waited to open the peritoneum till all blood had ceased to flow from the incision through the parietes, and he used animal ligatures, cutting them short and dropping in the pedicle; the patient was walking about in three weeks. Is it not marvellous that, with such a perfect example of good surgery before them, operators should have gone on blundering with long incisions, long ligatures, clamps, and abominations of every kind, piling up the victims till the total is probably equal to the death roll of a great battle?

Two years later Dr. Alban Smith, of Danville, Kentucky, operated, and marked another important step in the correct method of operating, by the pains he took to include the cut edges of the peritoneum in his sutures; his patient afterwards bore two children. He had operated unsuccessfully in 1818, and probably, as most of us do, if we have an open mind, had profited by the knowledge gained by the first failure.

Dr. Rogers, of New York, in 1829, again used short ligatures, tying the separate vessels in the pedicle, and closing the incision; and in 1835 Dr. J. Billinger, of Charleston, undaunted by a previous failure, operated successfully, tying the arteries in the pedicle with animal ligatures, cutting them short, and closing the incision.

In 1843 Drs. Dunlap and J. L. Atlee both performed their first operations; and this marks an important point in the history of the operation, for they are the first operators who may be said to have become specialists, each going steadily on and doing a considerable number of cases, their predecessors only having done an occasional case at long intervals.

Dr. W. L. Atlee, brother of the above, performed his first successful operation in 1849, and was afterwards for many years the leading ovariotomist in America. According to Peaslee, himself a well-known operator, and author of a valuable work on ovarian tumours, the total number of known operations in the States up to 1850 was 36, with 21 recoveries, a mortality of 41·66 per cent. Then in the next twenty years the total rose to 739 operations, with the terribly increased mortality of 70 per cent. I have little doubt, from a study of the records of this period, that the long ligature first, and later the clamp, were the chief causes of this lamentable failure.

The long ligature appears to have separated from the nineteenth to the thirty-fifth day, or even later, and during the whole of that period the unfortunate patient had a carefully prepared moist roadway for the causes of putrefaction and death, leading directly from the outer air to the deepest recesses of her peritoneum, and at the bottom of this roadway a lump of half-strangled tissue; and probably the dirty fingers of the nurse, and the still more dangerous fingers of the surgeon, fresh from other suppurating and putrid wounds, had a tug at it daily, stirring up fresh irritation in the remains of the pedicle, which nature was all the time doing

her best to heal, and leaving only too often in the moist silk thus handled, the most deadly causes of future mischief. Shall we marvel that there is a death-rate of 70 per cent., or shall we not rather marvel at the hair-breadth escapes there must have been, and at the marvellous vitality and resisting power of the tissues, of the lucky 30 per cent. who recovered?

Then we must remember that this does not represent the whole early history of ovariotomy, for what strikes one most is the enormous number of incomplete operations; probably this was largely due to the want of anæsthetics, and to the want of suitable instruments for dealing with adhesions and hæmorrhage.

Peaslee still, alas! far ahead of his time, writing in 1865, says: "Recent facts led to the conclusion that the treatment by the ligature cut short, and the pedicle left in the peritoneal cavity, as first practised by Dr. Nathan Smith, will prove to be the best method." Ten years later, when I first became thoroughly conversant with the methods employed by the then leading operators, I however found the clamp in general use, and the mortality only reduced to from 25 to 35 per cent.

I have dealt thus at some length with the work of the American operators, because on their soil the operation had its birth, and by them the young plant was feebly kept alive and nourished, though its offspring mostly perished; but I now pass to consider the history of the operation in our own country, where its greatest successes have undoubtedly been achieved, and where the operators, at last returning to the good teaching of Smith, Rogers, and Billinger, have been rewarded by a success as remarkable as is the long history of previous blindness and failure.

In the 'Philosophical Transactions' for 1724 is recorded a case of ovarian tumour operated upon by Robert Houston, of Glasgow, in 1701, but this is really an incomplete case, for he merely incised and cleared out the tumour, and left a tent in the lower angle of the wound for some days; the patient recovered, and was alive thirteen years later. Both the Hunters discussed favourably the possibility of doing ovariotomy, and, as Tait points out, they were born and brought up in the district where Houston's case was operated upon, and probably knew all about it. I have already alluded to the influence which John Bell's teaching probably had on McDowell, and he was a friend of the Hunters, so that, though we cannot credit Houston with the first successful ovariotomy,

we may well believe that from his venture the operation really sprang into being. Lizars attempted ovariotomy in 1823, but found no tumour, and publishing this case in 1824, he appended the record of McDowell's work, which the latter had sent to him several years earlier (1816). He performed ovariotomy successfully in 1825, using the long ligature for the pedicle. In the same year he had a fatal case with the short ligature. In his successful case he transfixed the pedicle. He had some other unsuccessful and incomplete cases, and the result of his work seems to have been, that the operation was thoroughly discredited in Scotland, and was not again attempted till 1845, when Dr. Handyside operated in Edinburgh. From this time up to 1862, the operation was rarely attempted, and only once with success. But in 1862, Keith appears upon the scene, and henceforth Scotland must take the foremost place in the history of successful ovariotomy; it seems in his hands to have mattered little whether the clamp, the short ligature, or the cautery were used—brilliant results were obtained by all; and, as I know from personal conversations with him ten or twelve years later, he sometimes thought the one method best, and sometimes the other; but I am anticipating, and must return to Keith's work later.

In the sister isle, the operation had been only three times performed up to this time, and always with a fatal result.

In England Dr. Granville operated in London in 1826 and 1827, but the first case was abandoned on account of adhesions, and the second proved to be a uterine tumour. Ten years later, Jeaffreson, of Framlingham, had a successful case, introducing the use of a very short incision, and the same year King and West had successful cases; the latter had another in 1839, and also a failure which is noteworthy as having been performed in a large London hospital—the London Hospital. In 1846 Mr. Cæsar Hawkins first operated successfully at St. George's Hospital. Up to 1842 there had been no successful case in London, though ten successful cases had been recorded in the provinces. In this year (1842) Charles Clay, of Manchester, commenced his operations, and saved three out of his first four cases, and in the same year Mr. Walne had the first success in London. Jeaffreson, writing in 1844, remarks that, "if not the originators of the operation, British surgeons at least deserve the merit of having placed it on anything like a sure foundation." I am afraid, however, the remark

was hardly justified at that time, for there had been no successful case in the metropolis, and only a few with many failures in the provinces. Frederick Bird, Aston Key, Bransby Cooper, and S. Lane, were among those who had the courage to perform the operation in these early days. To Charles Clay, however, must be given the credit of establishing the operation at this time, though I am afraid I cannot see any justification for the title of "the original hero" of the operation given him by Fehr. Undoubtedly he did much to overcome the opposition to the operation by his success, and by the full report of his earlier work; unfortunately he did not continue to give full and accurate reports of all his cases, and hence suspicions arose as to whether his success was maintained. He had 95 recoveries in his first 137 cases, a mortality of 30·65 per cent., a very respectable result when we consider the records of ovariotomy down to 1876, or thirty years after he began his work. The unfortunate hybrid term ovariotomy was first suggested to Clay by Dr., afterwards Sir James, Simpson, and has now become so much a part of the surgery of our time, that we can never hope to substitute a more correct term. Writing in the 'Transactions of the Obstetrical Society,' in 1863, Clay says: "I believe I have been mainly instrumental in bringing ovariotomy before the public, although I have had much to contend with, and have often been grossly misrepresented; still, in spite of every opposition, I have lived to see it established (under proper circumstances) as a legitimate operation in surgery, and practised by others with a fair share of success. Had I never accomplished any other great object in my professional career than this, I still fancy I shall not have lived altogether in vain, but have contributed something to the benefit of my species." Unfortunately, as I have said, he ceased to record his cases, and doubts arose as to the results, and the operation was again falling into somewhat evil repute, when, in 1858, Spencer Wells began to operate. At this time Baker Brown blazed like a comet across the field of surgery, dazzling the public and the profession by the brilliance of his success and operative skill, but sinking almost as quickly to obscurity and loss of good name. Unsuccessful in his early series of ovariotomies, *i.e.*, so long as he used the long ligature and the clamp—67 cases, 30 deaths, 44·7 per cent.—he became much more successful after he introduced his cautery clamp, reducing his mortality to about 11 per cent., and

this method must still be regarded as the most perfect theoretically, though, for various reasons, it cannot hold its own in practice against the short ligature. Though ovariotomy was practically born and nourished in its infancy in the United States, the surgeons there soon fell behind this country in the race, for there were in the latter 91 cases up to 1850, against the 36 I have already mentioned as being performed in the States up to that date. Of those 91 cases 33 died, a mortality of 36·27 per cent. Thus in this country the number of operations was not only three times as great, but the success was 5 per cent. greater.

It is difficult for me to speak of the work of my old master, Spencer Wells, and I must leave it to the future medical historian to assign him his exact place among the select few, who will have their names handed down to future generations, as the founders of one of the greatest works in surgery—great not only in its actual saving of valuable lives, and relief to human suffering in one particular form of disease, but far greater in the vast change it has wrought in all surgery, and in the ever widening field it has opened up in the special surgery of the abdomen. It is certain that the regular publication of all his cases gave the public and the profession a confidence they had never before felt, and greatly helped to sustain the operation at a critical period of its history, and I think it would have been well for surgery had his example in this particular been more followed with regard to other surgical operations.

Wells made his first attempt on December 29th, 1857, but finding the tumour behind the intestines he closed the incision, and the patient recovered, to die later under the care of another surgeon, of peritonitis, the result of tapping and rupture of cyst.

On February 19th, 1858, he performed his first complete ovariotomy, using the long ligature, which came away with a large slough on the twelfth day : the patient recovered.

In his third case he used the clamp; and I must record my opinion that this was a most unfortunate thing, not only for his own success as an ovariotomist, but also for the progress of the operation; or I would rather say it was the great cause of its want of progress, for, though plenty of cases were operated on, and a large proportion of lives saved by himself and others during the next twenty years, abdominal surgery practically made no advance

during this time; and this I attribute mainly to the heavy mortality attending the use of the clamp.

When he threw aside the clamp, and used only the short ligature, his results immediately improved enormously; and I firmly believe that if he had followed the example of Nathan Smith, from his start in 1858, his success would have been treble what it was. When I first saw his practice, in 1873, I at once took a great dislike to the clamp as a dirty, clumsy, and unscientific instrument: and I only used it in a few of my early cases, and, as my published tables show, with anything but success.

Keith commenced to operate in 1862, using, first, a ligature for the pedicle, but keeping the stump out with a pin, and in his next case using the clamp. His work is before the world, and no one can give it anything but the most unstinted admiration and praise. Tait followed five years later (in 1867), and was for a considerable time a very unsuccessful operator; indeed, a careful examination of his published tables, leads me to the conclusion, that his results only improved with the general improvement which took place in all abdominal work about fifteen years ago, *i.e.*, when antiseptics, in some form or other, came into common use, and the short silk ligature and the drainage tube became popular. It has always surprised me that he, of all men, should have followed, blindly, a method which he now so loudly decries.

Passing now from America and Great Britain to the Continent of Europe, we find Delaporte proposing the operation in 1774; and in 1798 Chambon said: "I am persuaded that a time will come when this operation will be extended to more numerous cases than I have proposed, and that it will not be found difficult to execute;" but nothing came of their advocacy, and it was not till 1844, when Chereau published a paper containing a collection of 65 operations with 42 successes—a mortality of only 35·38 per cent. —that Dr. Woyerkowsky performed the first successful ovariotomy in France; and so late as 1856–57, at a great discussion at the French Academy of Medicine, we find all the best known names in French medicine and surgery ranged in the opposition. The reading of the opinions then expressed is, in the present day, extremely amusing; but it was not so then to the poor victims of ovarian tumour, for it checked the progress of the operation in France for several years.

Koeberlé, of Strasburg, who has been one of the leading Con-

tinental operators, and to whom we owe the first suggestion of the glass drainage tube, and the valuable serre-nœud which bears his name, operated first in 1862, the same year as Keith; and it is worthy of note that to Keith we owe the modified and improved Koeberlé's tube, now in general use. The results in France up to 1867 were deplorable, however, for Boinet collected up to that time 122 operations with 73 deaths—a mortality of nearly 60 per cent. In Germany, Chrysmar, in 1819, first performed the operation, and this was the first case in Europe. His first and third cases died, and his second recovered. According to Dutoit, there had been in Germany, from 1819 to 1856, 64 operations—with the terrible mortality of nearly 72 per cent. It is not surprising that, with such a record before the profession, the operation did not make much progress in succeeding years, and even as late as 1870 the deaths exceeded the recoveries; but Lister has changed all that, and now the German operators run us very hard in their operative successes.

It would occupy me too long if I were to attempt to follow, even in the brief fashion I have adopted so far, the rise and progress of ovariotomy in other European countries, and in India, and the Colonies; it must suffice to say, that nowhere did it make even as good progress as in our own country, and in most places it was very slow in making any progress at all. Before considering in a little more detail the methods employed during these years of slow progress and occasional collapse, I will briefly refer to the operation for the removal of fibroid tumours of the womb, and to the few removals of the spleen, which are really the only abdominal operations, outside ovariotomy, which were ever attempted, before the great advance of fifteen years ago, at which period I propose afterwards to commence my remarks on present abdominal surgery. I do not forget that there were occasional operations for obstructed bowel, but they were so rare, and so commonly fatal, that they hardly deserve even a passing notice.

In 1861 Spencer Wells did his first hysterectomy for fibromyoma, treating the pedicle by a long ligature brought out of the wound; his first five cases all died. In 1870 he began to fix the stump in the angle of the wound, and in 1874 he used the clamp. His published tables show that his results were most discouraging, a mortality of 66·66 per cent. up to 1876. When we consider that the operation was often performed in cases which might have

lived on and recovered without operation, if they could only tide over the menopause, we cease to wonder at the strong opposition which the operation has encountered. Fibro-myomata are such a very different class of cases to the ovarian tumours, that a mortality which might seem justifiable in attempting to cure the latter, is altogether unjustifiable for the former, and here it is more than double as great. Keith did not perform hysterectomy till 1874, and then believed the tumour to be ovarian. Koeberlé's serre-nœud, which still, in my opinion, holds the field as the best means of securing the stump, was used. In 1885 he published thirty-eight cases with only three deaths, a brilliant series, when we examine the record and see how many desperate cases are included in it. After 1881 he frequently used a clamp with long thin blades. To this part of his work, however, I shall refer again in the second part of my address.

Wells removed the spleen three times before 1876, and in every case with a fatal result. He has recently had two successful cases. We have then up to 1876 an immense number of ovariotomies performed, with a varying mortality, according to the operator, but with a general mortality of from 30 to 40 per cent., a limited number of hysterectomies, and hysterotomies for fibro-myoma, with a much higher mortality, and a few other cases of an exceptional kind which were almost all fatal. What a ghastly record, when we contrast it with the present success in the much larger field of abdominal surgery as at present practised, though the latter includes a large number of operations incomparably more difficult, and more likely to be dangerous to life, than ovariotomy! What then were the causes of the different results obtained in the past, as compared with the present? I think I must bracket the long ligature and the clamp together, as the prime causes of disaster. What could be more unscientific, than to tie a great lump of tissue with a long string or silk, without special preparation of the material, and then to leave this ligature dangling from the wound? The tissues in the distal part of the stump were partly or wholly cut off from nourishment, and unless they very quickly acquired a fresh base of supply, by adhesion to neighbouring parts, were almost certain to slough. If they acquired a new attachment the separation of the ligature was long delayed, and during this time it was, as I have already explained, a constant source of danger. If they sloughed separation was quicker, but

the only hope for the patient lay, in the rapid encysting of the ligature and slough, to shut off the poison from the general cavity of the peritoneum, and by the time the slough was ready to come away, the external opening through which the ligature passed had closed round it, so that the slough could only escape by fresh breaking up of tissue, with its attendant risks of septic absorption.

To these risks, inseparable from the long ligature itself, must be added the risk that it became contaminated by the hands, instruments, or sponges of the surgeon daring application, and that the stump was also well smeared with every variety of septic organism. For there was no flushing with clean water, or bathing of hands and instruments in powerful germicides. On the contrary, every conceivable abomination was practised, for as late as 1874 I saw ovariotomy performed in one of our largest London hospitals, and the vomit and froth due to ætherisation, were wiped away from the patient's mouth with sponges, which were then washed in the same vessels with those used inside the peritoneum; if this was not as good an attempt at murder as the chance blow in a drunken brawl, then I hold views on such matters which I suppose I must regard as peculiar, yet many a poor fellow has suffered the penalty of the law for the latter, while the highly educated and scientific surgeon is the admired trainer of a host of our future surgeons. Now in what did the clamp differ from the ligature? Well in one very important particular; it brought the sloughing stump outside the peritoneum, though commonly only into the lips of the parietal wound, but it was infinitely worse than the ligature in other respects, for, with its great clumsy blade, it crushed a great mass of tissue, and rendered sloughing inevitable; the ligature, while it checks hæmorrhage at the time, does not necessarily, or even generally, cut off all nourishment from the distal part of the stump, as I have proved by careful experiment and dissection, but with the clamp, the very essence of its method was sloughing, and this sloughing only too often extended rapidly into the proximal part of the stump, below the actual grasp of the clamp, and then it at once had all the disadvantages of the long ligature. Moreover, it covered in the very part of the wound that ought to have been uncovered, so that it could be kept clean, and at every dressing there was necessarily more or less disturbance of the putrid tissues, and consequent risk of septic absorption. So

much for its method; but what of the actual instrument, with its great hinge, impossible to clean? It was taken off the pedicle of a patient dying of septicæmia, or it came away reeking with the foulness of a separating slough, and it was sent to the instrument makers to be cleaned, came back with its hinge oiled with the grease of an instrument maker's shop, in which probably post-mortem and dissecting instruments were cleaned, shall I not rather say dirtied, side by side with it, and then it was put on to a fresh pedicle without even a wash, or a boil, or a steeping in some antiseptic lotion. It was put on, I say, to a nice little pedicle, which a fine sewing needle and silk would have secured, and the patient, a beautiful girl, perhaps, in the first flush of womanhood, was in thirty-six to seventy-two hours a corpse, bloated and disfigured, with foul gases bursting from her tissues wherever they were incised, and then came the record too often "exhaustion" or "peritonitis." It is a terrible retrospect, and yet how many years this clumsy tool, which the commonest mechanic would at once have cast aside, was praised and recommended in every text book; and, so strong is the power of authority and reiterated error, that I fear it has not even now quite vanished from the field; indeed it is still commonly figured in the instrument maker's catalogue. The prevalence of the use of the long incision must also be credited with a share in the bad results of the early work in ovariotomy, for, while I hold strongly that it is bad practice in this as in any operation, to cramp oneself by a too short incision, and when dealing with extensive adhesions in ovariotomy, I believe that it is best to extend the incision, so as to get full view and control of parts to be separated, and bleeding points, I cannot but think, that the very long incision, once commonly used, must have added greatly to the shock of the operation, besides exposing the peritoneal surfaces much more extensively to possible causes of contamination. Long delay in operating and antecedent tappings were also causes of increased mortality, the former acting by its influence on other vital organs, especially the kidneys, and the latter by increasing adhesions, by lowering the general vitality of the patient, and still more disastrously by the frequency with which it led to putrefaction of the cyst contents, and consequent contamination of the peritoneal surfaces, pedicle, ligatures, and sutures, during the subsequent removal of the tumour. I am quite certain that a large share of the mortality of my own prac-

tice has been due to this latter cause, and that I thus suffer, unjustly, blame that should fall on the shoulders of those who tapped clumsily, and without due attention to surgical cleanliness, before sending the cases to me for operation. This must be the experience of everyone who has performed many ovariotomies.

While I thus dwell upon details which are special to the operation we are considering, I must not forget the general want of knowledge, as to what was meant by cleanliness in surgery. We have lived so much in the age which has been revolutionised by the example and teaching of my great master and friend Lister, that we are apt to forget the awful experiences of our early student days, when it was our privilege to hold up a stump, while the surgeon pressed out a stream of putrid pus, and the poor patient writhed with agony at the rough handling of the inflamed and irritated tissues. When one contrasts the reception in Berlin, and the grand appreciation of a great life work shown in Italy, with the good taste and scientific acumen of a recent address in our own country, we are indeed forcibly reminded of the words of Holy Writ, "A prophet is not without honour, save in his own country." It is a consolation, however, to know and feel, that history is just, and that the one name will echo through the ages, famous for evermore, as one of the greatest relievers of human suffering who has ever glorified our art, while his critic will be only remembered, at most for a generation, as a skilful operator in one special field.

To sum up then the causes of slow progress and too frequent failure in abdominal surgery up to 1876, and to place the various causes in what I believe to be their order of importance. We have first the general want of cleanliness and the lack of all appreciation or knowledge of what constituted surgical cleanliness, then the long ligature and the clamp, both as I have shown clumsy and unscientific, and both specially suited to make the want of cleanliness more deadly, and then following with an appreciable but far different influence, we have delay in operating, tapping, and the long incision. Then I must not forget drainage, for I think it is highly probable that a really good system of drainage, such as we now have, thanks to Koeberlé and Keith, would have done much to counteract the evils I have named above, though the frequent use of the drainage tube, with the long ligature and the clamp, would have introduced new elements of risk, which

I shall have to refer to again when I speak of the place which drainage occupies in the successes of to-day.

Let us turn then from the dark picture of the past, and watch the gradual breaking of the dawn of our present successes, revealing the general adoption of antiseptics, and an appreciation of perfect cleanliness; coincidently the general adoption of the short silk ligature, and the dropping of the pedicle; the more frequent use of the glass drainage tube, and the flushing of the peritoneum; and, last but not least, the recognition of the fact that it is folly to let a woman be half killed by a disease before you attempt to relieve her of it, and that it is equal folly to suppose, that she is more likely to recover quickly and well, in this half dead condition, than when in average health. Of course this error arose from the observation that simple cases so often died quickly from septicæmia, the explanation of this being partly, that in a robust person acute diseases generally run a rapid course, and also probably, that patients worn down by suppuration in the tumour and septic infection, as they so often were before operation, were not so susceptible to septic inoculation, of which nearly every patient operated upon had to run the gauntlet.

In 1876, I did my first hysterectomy at the Samaritan Hospital, removing the whole of the uterus except the cervix, and also both ovaries and tubes, and securing the stump by transfixion and ligature, after the method which I had for some time previously adopted entirely in ovariotomy. The patient recovered, but by good chance, rather than good surgery, as a large piece of the pedicle sloughed and was discharged through the os uteri into the vagina. This and unsuccessful cases in 1877 and 1878 convinced me that it would not do to ligature the stump, and I then tried the old ovariotomy clamp with success for a tumour weighing 62 lbs., having then not seen Koeberlé's serre-nœud; the clamp, however, caused considerable sloughing of the uterine tissues below its grasp in a case operated upon in 1880, and the patient died of septicæmia, and I have ever since used Koeberlé's serrre-nœud, whenever it has been possible to apply it, and with increasingly good results. I do no not think that this is by any means a perfect method, because, as with the old clamp, the stump has to separate by putrid suppuration and sloughing, and, though it is much easier to keep the parts fairly clean round the thin wire, it is risky always, every disturbance with change of dressing leading

to cracks and fissures in the granulating tissues round the separating slough, which may and sometimes do allow septic infection. Moreover, unless very great care is exercised to apply the sutures so as to leave the skin edges pretty loose round the stump, abscess is very apt to form in the fat and cellular tissue of the abdominal walls, and to burrow into the cellular tissue above the uterine stump, and in some few cases the pressure of the wire causes sloughing below, just as did the old clamp.

Theoretically Schroeder's plan of suturing the various tissues of the stump with successive layers of buried sutures should be the best, and I have had a few excellent cases by this method, when it was impossible to apply the serre-nœud, and I always use it now when the fibro-myoma can be removed without opening the uterine cavity. When this cavity is opened, as in an ordinary supra-vaginal hysterectomy, the difficulties of thoroughly cleansing the uterine mucous membrane and vagina, so as to certainly avoid sepsis, are in most cases insuperable, because one cannot efficiently control hæmorrhage without closing the very channel one has to clean. If any method could be devised by which the vessels in the stump could be temporarily, but efficiently, secured, the cavity of the uterus being at the same time kept quite free and open, so that one could thoroughly clean it right through into the vagina, and then pack the latter with some efficient dry antiseptic material, which would keep this passage aseptic without risk of poisonous absorption, the days of the serre-nœud would be numbered, and the success of hysterectomy greatly increased.

In some cases it is possible to leave one or both ovaries, and when this can be done, especially in young women, I think it is right to leave them ; the few cases in which I have been able to do so have recovered just as well, or better, and it is an immense relief to them to hear afterwards that they are not altogether unsexed. One would have rather expected that such cases would have menstruated from the remains of the uterus, but none of my cases have done so, and none of them have experienced any inconvenience at the month from the ovaries, while they seem to me to have to some extent suffered less from the flushings, perspirations, &c., which are such a source of worry, to most patients, for a longer or shorter period after the removal of both ovaries. These cases seem to offer some support to Tait's theory as to menstruation not being in any way dependent on the ovaries, but they are at present too

few to argue from, and as a set off to them, the only case in which I have ever completely removed both tubes, leaving both ovaries, goes on menstruating quite regularly, though with, if anything, more pain and discomfort than she had before the operation. The operation was for double pyosalpinx. Before leaving the subject of hysterectomy for fibro-myoma, I must refer to those cases in which the tumour has been developed in such a direction that it lifts up the pelvic peritoneum, either anteriorly, laterally, or posteriorly, or in some cases in all directions at once. These tumours are generally very large, and on first opening the abdomen it seems absolutely hopeless to attempt their removal, but a knowledge of their method of vascular supply, makes them in reality, not nearly so formidable as they look; but few vessels enter the substance of the tumour, and these through the uterine wall close to the junction with the cervix, the rest of the vessels ramify in two layers in the capsule of the tumour; first, there are the ordinary ovarian vessels, with their anastomoses from the uterine arteries enormously enlarged, and then there is a second series deeper in the capsule, the exact source of which I have never been able to make out. It is obvious, then, that if the capsule can be separated with these two sets of vessels, and the tumour cleared down to its base, this base can be dealt with by the application of the serre-nœud, as in an ordinary hysterectomy, the only difference being that the serre-nœud is applied to the uterus at the junction of the body and cervix, inside its peritoneal investment, instead of outside it. The vessels in the broad ligaments and capsule are, as I have said, enormously enlarged, and lie in two separate planes, but chiefly on each side, and over a limited area, so that, if their situation is seen, the capsule can be divided on each side of them, and they can be secured temporarily by large clamp forceps, or strong ligatures, while the other steps of the operation are completed, and afterwards tied by transfixion, as in ordinary ovariotomy, or removal of the appendages. It is a question which can only be settled in each individual case, and by the experience of the operator, whether these vessels shall be first secured, or whether most of the capsule shall be separated from the tumour before they are secured. I much prefer the latter plan, whenever it is possible, and have adopted it with success in some of the largest tumours I have enucleated. If we tie the vessels first, there is an enormous volume of blood stagnated in the tumour,

as it is pumped in through the vessels already referred to as entering at the base, and prevented from returning into the general circulation by our ligatures, the result being that when the tumour is removed the patient is deprived of an immense quantity of blood, and the after-steps of the operation are, moreover, obscured and rendered very difficult by the constant escape of this dark blood over the parts the surgeon is separating. Whatever plan is adopted, it is of the first importance that the enucleation should be commenced at some one point, and steadily proceeded with in one direction, and with definite aim. Tait has pointed out the importance of this in the enucleation of cysts from the broad ligament, and it is even more important here.

The surgeon must also, before beginning such an enucleation, with an enormous solid tumour weighing perhaps 40 to 60 lbs., have the most perfect confidence in his own nerve, coolness, and readiness of resource; a minute's hesitation and indecision at a critical moment, may allow such hæmorrhage, that the patient is lost. I believe, as the result of some considerable experience in this particular class of case, that the enucleation should always commence by a straight incision right across the face of the tumour just above the bladder, and the latter should then be perfectly peeled back, so that its exact relation to the base of the mass is seen, and a reasonable notion obtained as to the probable situation of the ureters; if this is not done, the bladder is very apt to be torn at a later period of the operation, when the parts have become confused by extensive enucleation. This incision will often be 10 or 12 inches long, and will require the rapid application of a couple of dozen pairs of pressure forceps to the divided vessels on its sides. Having completed the separation of the bladder, the next step is to make a similar cut across the capsule, at the back of the tumour, and separate it posteriorly, lifting the mass which generally fills the true pelvis, which it has completely stripped of its peritoneum; and during this procedure great care must be taken not to damage the ureters, iliac vessels, rectum, sigmoid flexure, or cæcum, all of which are in very close relation to the tumour. When this is done, we have the tumour attached at its base, and at its two sides. The finger should now be carefully insinuated close to its base under the deeper layer of the lateral vessels, and through the channel thus made the wire of the serre-nœud should be applied round the base,

and lightly screwed up; a large portion of the arterial supply is now cut off, and, the large veins in the pampiniform plexus being still free, the tumour rapidly loses most of its blood, and by the time the broad ligaments, and the deeper layer of vessels on each side, have been secured, the broad ligaments and base can respectively be rapidly cut through, and the tumour removed without the enormous escape of back blood from the tumour, which often renders this part of the operation so difficult, when the ligatures have been applied to the broad ligaments too soon.

I know of no operation in surgery which requires more exact knowledge of the thing to be done, and the way to do it, and I know equally of none that gives one a greater feeling of relief and satisfaction when it is safely concluded. Our late lamented friend Dr. Matthews Duncan, whose sudden and premature loss we all so deeply deplore, has often expressed to me his profound admiration and wonder at the rapid and successful completion of such an operation, and he was not easily led into commendation of any operation.

Let me add here one word of caution, lest the neophyte in surgery should feel fired with the ambition to feel the glow of satisfaction I describe. No man is justified in attempting such an operation till he has had a large experience in the more simple procedures of abdominal surgery.

Passing now to consider the treatment of diseases of the ovaries and tubes, other than ovarian tumours, one is at once amazed that no attempt should have been made to deal with such cases till Battey, Hegar, and Tait all began to work in this special field, about 1872. Battey's original idea was to remove ovaries, not in themselves diseased, for the cure of certain nervous diseases, which he believed were caused, or kept up, by real or functional derangements of the ovaries. Hegar must have the credit of introducing the removal of the ovaries for the cure of fibromyoma uteri, while to Tait belongs the introduction of the operations for diseased ovaries and tubes. The number of operations in this field has been so very great, and so out of proportion to the number of ovariotomies for tumours, that one cannot but suspect that diagnosis has not always been very accurate; certain it is that many of the cases operated upon remain in worse health afterwards than before, that others are no better, and that a large number travel about from the consulting room of one specialist to that of

another, bewailing the loss of their ovaries, and urgently demanding relief from the discomforts which are incident to the abrupt production of change of life. I think that every close observer must see that one reason this class of operations did not earlier take the field was that there were not nearly the same number of women seeking relief for the miseries of inflamed and adherent ovaries, and tubes blocked by inflammation, and filled with serum, blood, or pus, before the gynæcologist, as he is known to us to-day, appeared with all his awe-inspiring array of instruments, and every pelvic ache or pain, whether in the matron or the maid, at once became an excuse for tenting, dilating, scraping, burning, and alas! too often slaying. My own consulting practice convinces me, beyond any doubt or question, that far more of the diseases for which Tait's operation is necessary are due to unskilful and often improper treatment of slight uterine disorders, than are due to true pathological conditions arising from accidents, chills, &c., during menstruation, and during unassisted labour; and here I must also record my belief that much of the disease arising among child-bearing women is due to the too great prevalence of instrumental delivery.

I must return for a moment here to the subject of fibro-myoma uteri, to say that I believe a very large number of cases can be traced to injuries inflicted on the uterine tissue by gynæcological and obstetrical instruments. Since I have returned to the subject of uterine tumours, I would say a word about electricity in the treatment of these tumours. I have never used it myself, but I have seen a number of cases which have been treated by others, including all those who have made it a specialty, and I am bound to say that I think it is an utter and complete failure. I have seen a few cases in which the tumours have appeared to shrink for a time; but I have seen quite as many shrink as much under a judicious regulation of diet, and with a carefully watched administration of ergot of rye. I have, moreover, seen several cases in which the tumour has considerably enlarged as the result of the electrical treatment, and in which the patients have suffered so much from it that they have urgently demanded operative relief, and in several such cases I have either removed the appendages or the tumour by hysterectomy, with completely satisfactory results. On examining two of these tumours after removal, I found them curiously softened and altered, and full of vessels

blocked with clot and extravasations of blood. It is then, I have no doubt, by these violent effects on the circulation in the tumours, that electricity cures, when it does cure, and if it ever does cure; but the operator has no possible means of regulating the amount of damage caused, or of knowing when he has done enough to hope for re-absorption of the tumour. Hence he too often goes on till the whole mass sloughs, and the patient either dies of septic-æmia, or escapes by the skin of her teeth, after a desperate illness and the extrusion of a large sloughing mass *per vias naturales*. When I first heard Apostoli introduce the subject at the Copenhagen Congress, I said to myself, this is nothing new; it is only a revival of an old method in an amended and slightly more scientific form, and I don't believe it will do much for us in the treatment of these diseases. I have carefully watched the progress of the whole business, and the work of its re-introducer, and if it is not already condemned by the general voice of the profession as a failure, then I am sure the day when it will be is not far distant. Many of the so-called cures have been simply due to blunders in diagnosis, leading to the puncture of ovarian cysts, or other fluid collections in the pelvis, in mistake for fibroids. One case of my own was subjected to the treatment, because she had some pretty severe hæmorrhages after the removal of the appendages for the cure of fibro-myoma, but the case has been seen since, and I have heard her whole history, and I am convinced, from my experience of similar cases, that all that she got from the treatment she would have got by patiently waiting till the full effects of the removal of the appendages were obtained. I may as well, while on this subject, say all that I have to say about the removal of the ovaries for the cure of fibro-myoma. We owe an immense debt to Hegar for the introduction of this method of dealing with fibro-myoma; it, of course, has its risks and its failures, but with care in the selection of the proper cases, and care in the removal of every particle of ovarian tissue, it is most satisfactory in its results, and I consider it one of the most thoroughly scientific and valuable operations in the present field of abdominal surgery.

After this long digression, let me return to the question of the removal of diseased ovaries and tubes. I have stated my conviction as to the causes which render operation necessary in by far too many cases, and I think we may fairly hope that, as the medical student becomes better educated in the diseases of women,

this class of cases will greatly decrease; but there will always remain a certain number of cases, arising from what I will call legitimate pathological conditions, and there is a large field here for the truly beneficial exercise of the abdominal surgeon's art. It is, however, a class of cases requiring great diagnostic skill and great judgment, as to when it is right to operate, and when to hold one's hand. When I look at the enormous lists published by some operators, I feel that I am a very baby in this field; but I know that I have myself operated on cases which might have got well without operation, and I am more and more inclined to persevere with the administration of ergot, and the use of the hot douche, for prolonged periods, before resorting to the knife. I believe both these agents have the same action, *i.e.*, they regulate the circulation in the pelvic viscera, and prevent those engorgements and stagnations which are so harmful, when once the appendages become inflamed and displaced. Some women can bear to have their ovaries chronically inflamed, and even also displaced, without suffering more than slight increase of the flow during menstruation, and some extra discomfort during, or just before, and just after, the flow. Others are made complete invalids by the same condition, and their whole usefulness in life is destroyed, and they become a burden to themselves and to their friends. In such cases, especially if the ovaries are firmly adherent, I believe we are just as much bound to give relief by operation, as we are to cure by the knife any other painful disease which is amenable to surgery. If we could differentiate between hydrosalpinx, hæmatosalpinx, and pyosalpinx, then I think we should not be justified in operating in the former, should wait long and watch the case carefully before deciding to operate in the second class of cases, and should always operate at once in the latter; and I think, as we cannot surely diagnosticate each case, the danger the patient runs from unrelieved pyosalpinx is so great that when we have good reason to think the latter condition is present, it is better to operate than to delay, even at the risk of occasionally removing a hæmatosalpinx, or even a hydrosalpinx.

Before leaving this subject, I would point out that there is a vast difference between these operations. Inflamed and firmly adherent ovaries with fairly healthy tubes, hydrosalpinx, and even hæmatosalpinx, may be removed with a very trifling risk; but there is no more dangerous and difficult operation than the removal of closely

adherent tubes when filled with putrid or specifically diseased pus. And, quite apart from the danger of infection of the peritoneum and septicæmia, another danger attends these cases, and that is, that the intestines are especially liable to be closely adherent, and not only closely adherent, but softened, and in a condition very likely to tear, or, if they do not actually tear at the time, perforate shortly after, and hence a fæcal fistula is one of the dangers to be dreaded in operating for pyosalpinx, and, though by no means an incurable condition, it is a very serious one. I am certain from my own experience, and from watching the work of others, that if the mortality could be obtained for all the cases of pyosalpinx operated upon in the United Kingdom since Tait introduced the operation, it would run what we may call the natural mortality of the disease very close indeed. Then we must not forget that an immense number of the women who escape with their lives are not in any proper sense cured, for they remain martyrs to all sorts of pelvic pains and discomforts for months and even years after the operation.

The direction in which this class of cases wants improvement most is in the method of operating. The making of a very small incision and the scraping out of the diseased parts with the finger tips is a thoroughly bad and unscientific form of surgery. As in all other surgery, the incision should be free enough to give us all the aid which accurate sight as well as touch can give. Then the diseased parts should be gently and thoroughly freed from all but their base of attachment, and this should be tied off with the finest silk that can be used, and in small portions, not in a big lump, and the distal portions of the stumps should be as short as is at all consistent with safety. This is the direction in which I have been working in this class of cases, and I am sure the condition of my patients soon after operation, amply repays me for the extra time and trouble in operating. I am constantly seeing patients who have been successfully operated upon by other operators, who have in their pelves great lumps of tender inflamed tissue, which are evidently the stumps and ligatures remaining after the operation has long been performed. Such cases often suffer as much or more than before, pelvic pains, inability to walk, pain on defæcation, flatulence, and obstinate constipation, being among their most frequent troubles. It is among these cases too, that we find most frequently menstruation or, more

properly speaking, irregular metrostaxis after removal of both ovaries.

In 1879 Tait performed his first abdominal section for pelvic abscess, and for this suggestion we owe him undoubtedly much. I know of no more miserable class of cases than pelvic abscesses allowed to open by nature, or opened through the vagina or rectum by the surgeon, and one of the most satisfactory cases I ever had in practice was a case of pelvic abscess following confinement, which I insisted on freely opening by abdominal section, though it had during the night before I operated begun to discharge slightly into the bladder and vagina. The patient was the daughter of one of our best known general physicians, now, alas! no longer with us, and I met him and Dr. Playfair in consultation on the morning of operation, and had great difficulty in persuading him that it was still right to operate. My view was, however, supported by Dr. Playfair, and my old friend often thanked me afterwards for the long misery spared to his daughter by my firmness in advocating prompt abdominal section.

I feel that I owe some apology to these among my audience whose interest does not centre in the pelvic organs of the female, for I have occupied more than half my space with the surgery of these organs, and I must still say one word about uterine cancer. It is clearly demonstrated that there are a certain number of cases in which it is right to remove the uterus, and it is equally demonstrated that this is not an operation properly belonging to abdominal surgery, so I shall content myself with saying that I believe the abdominal method is or should be entirely abandoned for the vaginal, and that the cases in which the latter operation is justifiable are very few, *i.e.*, it should not be undertaken when the uterus is still absolutely free, and when the disease is thoroughly confined to the inside of the cavity, or has at most only slightly affected the cervical canal. Cases in which the os is affected either primarily or secondarily are only suitable for partial operation, such partial operations being infinitely less dangerous, and just as likely in this class of cases to give the patient a respite from the disease, as is the complete extirpation of the organ.

My own experience of complete extirpation is small, when compared with that of many of our Continental brethren, and I think the operation has not taken root, either here or in America, as it has on the Continent. Such experience as I have had leads

me to think that improvement in results, both immediate and remote, is to be sought by separating the tissues slowly and patiently bit by bit, applying forceps to the bleeding points, or tying them at once, as is most convenient at the time, rather than by tying the broad ligaments in large pieces and by deep transfixions. By the slower method one is much better able to avoid danger to the ureters, runs less risk of septicæmia, as there are no masses of tissue liable to slough, and is also better able to estimate the exact condition of the successive portions of tissue as one divides them.

I have once successfully removed a cystic spleen, and I have twice removed much hypertrophied spleens with fatal result; in both the latter cases a most curious condition of the circulation was observed as soon as the enormous vessels entering the spleen were secured: the patients became cyanotic, and all the small vessels which had been divided in the abdominal wall, and in adhesions, commenced to ooze dark blood, this oozing continuing after every possible effort had been made to check it by numerous fine ligatures, close suturing of the wound, and the application of styptics. In the first fatal case this condition was only transitory, but the patient bled to death through the retraction of a small vessel in the omentum, the hæmorrhage going on slowly between the layers of the omentum and being only discovered at the post-mortem; comparing this case with the second, this concealed hæmorrhage appears to have led to the cessation of the dark oozing, for in the second case the oozing continued from the exterior of the abdominal incision, and, I have no doubt, into the peritoneum as well, though I had no opportunity of examining after death. I made one curious observation in this case: I used styptic colloid to try and check the external oozing, and when it was freshly applied it at once frothed on admixture with the blood, and this coating of froth seemed to have no styptic power, the oozing going on under it unchecked.

I am convinced that there is some great change in the blood in these cases, which only becomes active when the blood is cut off from flowing through the large spleen, but I am quite at a loss to account for the condition. It was present to some extent when the vessels were first secured in the successful case, but passed off before the abdomen was closed, returning from time to time for some days after the operation during convalescence, and causing

attacks of dyspnœa with dark flushing of all the skin surfaces, and accompanied by pain in the region of the tied splenic vessels.

I have never removed a pancreatic tumour, and the successful cases are so few that they are rather curiosities of surgery than operations likely to teach us much. I have on two occasions had very troublesome hæmorrhage from tearing the pancreas in separating renal tumours; in both cases I had to pass fine needles, armed with silk, into the substance of the gland in order to stop the hæmorrhage; both cases recovered well, and were apparently not in any way injuriously affected by the interference with the pancreas. In one case of operation for gall-stone, in which the parts were much matted together, I got into the head of the pancreas by mistake, and made a hole of some depth, from which the bleeding was very free, but stopped spontaneously; I afterwards completed the cholecystotomy, and the patient made a good recovery.

The surgery of the liver is one of the departures in abdominal surgery which has made, and is still making, marked progress. I am not sure who first systematically attacked hydatids of this organ by abdominal section, thorough cleaning out of the cavity, and drainage, but I have had a considerable number of successful cases, some of which I have had the honour to report to this Society, and several remain to be published. It is very remarkable how these hydrated cysts of the liver form pouches deep in remote parts of the abdomen and pelvis, which keep in communication with the parent cyst, and will drain through it and cure, without separate opening. One such case in my own practice formed a large tumour in Scarpa's triangle, and another a large pelvic tumour simulating ovarian cyst. The operation of cholecystotomy we owe to the late Dr. Marion Sims, for, though his first and, I believe, only case was unsuccessful, he laid down with admirable clearness and precision the steps of the operation in the account of this case, which he published in the 'British Medical Journal,' 1878.

This is now a most successful operation, even when it is undertaken at a late period of the illness, and after repeated inflammatory attacks have caused adhesion, and even suppuration, in the neighbourhood of the gall bladder. I have now had many cases, and have only once failed to find and remove the stone or stones. This was a most disappointing case, for I felt certain of my

diagnosis, and found a much thickened and adherent gall bladder, which I emptied clean out and sutured, but could find no stone. The patient died of peritonitis, evidently from separation of the sutures in the inflamed tissues of the gall bladder, and the stone was found lying loose in the peritoneum, close beside the gaping wound in the gall bladder. How I missed it I cannot imagine, for I made a very prolonged and patient search. I think I may take to myself the credit of a new departure in this class of cases, which will, I believe, be found a valuable addition to our methods of dealing with impacted stones. I refer to the incision of the common duct, removal of the stone, and suture of the duct without interference with the gall bladder. I have now performed this operation three times, and in all three cases with a perfectly successful and satisfactory result. The great difficulty of the operation lies in the depth at which the duct lies, and the propinquity of such important vessels as the aorta and vena cava; the incision into the duct should be longitudinal and on to the impacted stone and it has to be made entirely by touch, the knife being quite out of sight, and guided along the left index finger resting on the stone. The suture of the opening in the duct is not so difficult as one would expect, and is performed by a long fine curved perineum needle, and fine interrupted silk sutures.

The cases in which this operation is necessary are those in which the stone, or stones, have passed entirely through the cystic duct, which has returned to its natural calibre, but with its walls thickened and hardened by the passage of the stone, so that it is impossible to dilate it sufficiently to withdraw the stone through it into the gall bladder. In one case, after free incision of the common duct, I was unable to extract the stones, and had to content myself with needling them well up, and then suturing the duct over them, left the fragments to find their way into the duodenum, which they did in the course of some days, and the patient, an elderly woman, made a good recovery. It might seem, at first sight, better, in such cases, to needle up the stone without opening the duct, or to crush it *in situ* with padded forceps, as suggested by Tait; but the needling is a very difficult and dangerous business without opening the duct, and the use of padded forceps also very difficult and dangerous in such a situation.

There is nothing in surgery like clean cutting, especially where

one is surrounded by large vessels, in which a chance puncture, or crush, may cause concealed hæmorrhage, or after-sloughing of the most disastrous kind.

In ordinary cholecystotomy, there can be no doubt that it is better, whenever possible, to sew the cut edges of the gall bladder into the abdominal incision, and drain, though the fistulous opening is in some cases very slow in closing, and very troublesome. When the gall bladder is so much damaged during the operation, or so friable from old inflammatory attacks, or so deeply held in the peritoneum, that this procedure is impossible, we have a choice of three operations. We may entirely suture the opening in the gall bladder and drop it in; we may leave the gall bladder open in the peritoneum, and bring a rubber tube out of it through the external wound; or we may entirely remove it, cholecystectomy. Each and all these methods I have practised with success, and I believe each has its advantages in individual cases. When using either method, it is advisable to pass a glass tube through the parietes, just above the pubes, and drain the pelvis, by the rubber suction tube and syringe, for the first few days, as in ovariotomy, in case there should be any overflow of bile or mucus from the gall bladder into the peritoneum.

The leaving the gall bladder freely open deep in the peritoneum appears, at first sight, to be a very dangerous proceeding; but, having been compelled to do it in one case, and having observed how satisfactorily it drained through its rubber tube, and how little fluid came from the pelvic tube, I was encouraged to repeat the procedure in another case, in which any other procedure would have been very difficult or impossible, and again with complete success. I hope to bring these cases before the Society in more detail shortly. My experience in the treatment of abscess of the liver has been comparatively small, but very successful, and I see no reason to suppose that abscess differs in this situation from abscess in other parts of the body; free opening, free drainage, and avoidance of putrefaction are the essentials for rapid healing. I may briefly recall one case (already published) in which, finding it necessary to open a large hepatic abscess through a healthy pleura, I did so with complete success by sewing the parietal and visceral layers of the pleura together round a lozenge-shaped opening between the ribs, and thus making a safe aqueduct for the passage of the pus through the diaphragm and pleura.

The operation for establishing a permanent communication between the gall bladder and the intestine, cholecystenterostomy, is one of the recent additions to our methods for dealing with closure of the bile ducts by inflammatory or other deposits, and may, in a limited number of cases, be found useful, but should, I think, never be allowed to take the place of the other operations we have been considering, when the obstruction is due to the presence of stones.

Tumours of the supra-renal capsule are now known to be not very uncommon, and to attain a very great size without necessarily implicating the kidney; two large tumours of this kind I have myself successfully removed, only to learn their true nature, through the kindness of Messrs. Eve and Targett in investigating their minute structure. A careful study of the history of a large slow-growing tumour in this situation might now lead one to form a correct diagnosis: the absence of any symptom of implication of the renal tissue, as evidenced by repeated and careful examinations of the urine, being the important differential diagnostic; and I think it probable that, in some exceptional cases, it may be found possible to remove the tumour and leave the kidney. The difficulty in this procedure lies in the fact that the same vessels supply the tumour and the kidney, and that they become enormously enlarged; still, I can conceive a case in which the vessels might be secured, after they have broken up, to supply the tumour, and the main trunk passing to the kidney be left undisturbed. I have so recently had an opportunity of laying my experience, and the views founded on it, in renal surgery, before the profession in my Harveian Lectures, that I do not propose on the present occasion to do more than briefly indicate the stages of advance in this department of the surgery of the abdomen, and the names of those to whom, I think, we owe these advances. Writing, as I have done, in the country away from any good library of reference, and from my own medical books, I have been dependent upon my friend Mr. Malcolm for names and dates, to a great extent, in this and in other portions of my address, and I have pleasure in taking this opportunity of thanking him for valuable assistance rendered on this and many other occasions.

The early nephrectomies were all performed on a mistaken diagnosis, and were uniformly fatal. A few rare attempts at

removal of renal calculus from a suppurating kidney, or from a perinephric abscess, shared the same fate.

In 1869 Simon successfully removed the kidney by lumbar incision, for wound of ureter, and in 1871 he had an unsuccessful case for calculous pyelitis; to his careful researches and work, much of the after success of renal surgery is undoubtedly due. Langenbüch had a successful nephrectomy by lumbar incision in 1875, but was not clear whether it was sarcoma of the kidney or of the lumbar muscles. Jessop, of Leeds, followed with a successful case for cancer in a child of $2\frac{1}{2}$ in 1877.

Campbell, of Dundee, had a successful abdominal nephrectomy in 1873, on a mistaken diagnosis of ovarian cyst. In 1878 I had a successful lumbar nephrotomy, but on a mistaken diagnosis, believing the suppuration and pain to be due to stone, and finding it to be tubercular. In the same year Weir had a successful case for hydronephrosis, and Muller, of Oldenburg, had a successful lumbar nephrectomy for calculous pyelitis, while in the following year Czerny had a successful lumbar nephrectomy for pyonephrosis and fistula, and in 1880 John Couper another for pyonephrosis. In 1881 Barwell had a successful case for calculous pyelitis and fistula. In 1873 Ingalls, of Boston, successfully removed a calculus from a suppurating kidney by lumbar incision, and in 1880 Morris performed the same operation for calculus without suppuration, Beck, Butlin, and John Duncan following his lead in the following year.

In 1880 I performed abdominal nephrectomy on a child of 7 for hydronephrosis, and followed up this first venture by ten more successful cases of abdominal nephrectomy for various diseases before I had a fatal case in 1885.

In 1884 I first proposed, and performed successfully, the removal of calculus from the kidney by combined abdominal and lumbar incision.

In this brief record of the early operations, I may have left out some, or have failed to give due prominence to others; for these omissions, if such there be, I plead want of time and space. The value of these isolated cases is to be found in the lessons they have taught us, as to what can be done with success in a new field of work. I am convinced that the future of renal surgery, both as to accuracy of diagnosis and success in relieving suffering

and saving life, lies mainly in the domain of abdominal surgery; much successful and good work may be done by lumbar incision, but it can never advance our knowledge in anything like the same way that abdominal exploration can. I should like to discuss at some length the recent work in intestinal surgery, more especially the contributions to our knowledge made by Senn, Parkes, Treves, and Jessett, but time will not allow it, and I must leave it to some future President to deal with this important portion of abdominal surgery, after the debate, which we hope still to have upon it, has furnished further material for theory and practice. I will merely say that our chief endeavours should be directed to the early differential diagnosis of the various forms of obstruction, because upon this entirely depends the question in each case, whether it is right to operate, and whether there is a fair chance of success. Thus, if I could always with certainty diagnose volvulus, I would urge immediate operation, because no case recovers without it, and with such prompt interference I should expect a large share of success. So again with constricting bands, apertures, or adhesions, I should urge immediate operation, because the gut is so apt to become gangrenous: in fact we are simply dealing with an intraperitoneal strangulated hernia; if, on the other hand, I diagnosed intussusception, I should be inclined to wait and watch, unless I was quite sure the case was perfectly fresh. This form of obstruction occupies a sort of intermediate position between the acute forms named above, in which early operation gives the only chance, and the more chronic forms, due to obstructing growths, abscesses, and strictures, a class which often recovers spontaneously, and in which I believe the surgeon is generally wise to hold his hand, and trust to atropine and morphia, leaving the cause of the obstruction to be dealt with after the acute stage is recovered from, and when the patient is in a better condition to bear a serious operation. Suppose that we could have a table of all the cases of obstruction which have recovered spontaneously, another of all that have died without operation, and likewise tables of all the successful and all the unsuccessful operations, I believe nature would run surgery hard, and would be out and out the victor if cases of volvulus, band, and aperture were excluded. I believe, however, that the balance of recoveries would be in favour of careful surgery, if only a correct

diagnosis could be made early, *i.e.*, before the case has been subjected to the injudicious use of purgatives and enemata.

I should like to have said something on gastrostomy and the other operations on the stomach, but time will not allow it. Of gastrostomy for the removal of foreign bodies I have had some successful experiences, of other operations on this organ I have none. The operations of Loreta and Woelfler (gastro-enterostomy) seem to me highly scientific and full of promise; for pylorectomy I have no sympathy—it is a desperate operation, and its results are, in my opinion, too poor to warrant its performance. I do not believe that any malignant disease that has once involved the peritoneal covering of an organ contained in the cavity of the abdomen, or more or less projecting into it, is likely to be cured by the surgeon's knife.

Time will not allow me to deal with such fascinating themes as supra-pubic cystotomy, the operations for the removal of ectopic pregnancy, of which I have had a fair experience, the Cæsarean section and its various recent modifications, Porro's operation, and the surgical treatment of tubercular and septic suppurations in the peritoneum. The mere mention of them shows how rapidly progressive is the abdominal surgery of to-day. I may say that I in the main agree with Tait as to the pathology and treatment of ectopic pregnancy, though I firmly believe in the possibility, nay, probability, of ovarian and abdominal as distinguished from tubal cases, though I admit that the latter is the common variety. As in so many other pathological conditions of organs contained in the abdominal cavity, it is to correct early diagnosis that we must look for more precise and certain pathology and for success in treatment. If diagnosis could be made with certainty before rupture, I would operate in every case; I do not believe that, with our present knowledge of the dangers of the condition, we are justified in risking the life of the mother by waiting for the very doubtful chance of saving a viable child. When once rupture has taken place it is equally the duty of the surgeon to operate, but each case must be carefully judged on its own merits, as to whether the operation shall be immediate, or delayed till the patient has rallied from the first hæmorrhage. I believe the great majority of cases do rally from it, and then I think it is better to wait; in some few it may be necessary to operate at once to save

the woman from bleeding to death. Now, by a few brief references to the statistics of well-known operators, let me justify my selection of 1876 as the turning-point in the advance of abdominal surgery. Wells had, up to the beginning of that year, performed ovariotomy 737 times, with a mortality on the whole series of a fraction under 26 per cent.; in 1875, with 66 cases, his mortality was still 22·7 per cent.; in 1876 and 1878, the clamp still being a good deal used, but not nearly so much, the percentage drops to 18 on 151 operations; and in the next two years, when Lister's method is adopted and the clamp is practically abandoned, there are 88 cases, with only a fraction over 8 per cent. mortality.

I have already given the results of his hysterectomies down to 1876: mortality over 66 per cent.; in the next five years, it dropped 20 per cent., and there was a further drop of 10 per cent. after he adopted Lister's method.

I commenced to assist Mr. Wells in 1873, and began abdominal operations myself in 1874; in 1874 and 1875 I only did an occasional case, and, following strictly the teaching of the day, made but poor work. In 1876 I definitely abandoned the clamp, and by so doing reduced the mortality of my ovariotomies 4 per cent. In October, 1877, I began to operate with the spray, and after the method then known as Lister's; the result of this was a further reduction of my percentage of 9·35, or, in other words, the abandoning of the clamp and the adoption of antiseptics lowered my mortality nearly 14 per cent. I have continued to use the spray and the carbolic lotions of the same strength as at first adopted, and I use the drainage tube oftener than I did; but I have only once or twice flushed out the peritoneum with water, and of my last 106 ovariotomies I have lost two, one dying in a few hours of acute peritonitis, which was present when I operated; and the other, an old lady of 69, of bronchitis, acquired from carelessness, when she had practically recovered from the operation. I am not ashamed still to use the spray, and all the precautions which have advanced my results in ovariotomy to 1·88 per cent. mortality, and I find increased practice and a steady adherence to methods which have yielded me good results in the past, increase in like ratio my success in all abdominal operations, whether upon ovary, tube, uterus, kidney, liver, or gall bladder.

Tait's mortality, in his first 50 non-antiseptic cases, was 38 per

cent., and on 176 non-antiseptic cases 13·95 per cent.; then after I had been some time at work with antiseptics, in 1879, he took them up, and had 50 cases with only 3 deaths, only 6 per cent. Thus there was a marked improvement from his early series when he used the clamp chiefly, to his further non-antiseptic work with the ligature, and a still more marked improvement when he took to Listerism; he then learnt for the first time what surgical cleanliness meant, and he has never forgotten the lesson, though he has chosen to adopt other methods of attaining it than those then used by Lister, just as Lister himself constantly varies his method.

The most recent record I can find is in the 'British Medical Journal,' November 17th, 1888, when he published 1,000 abdominal sections, and, so far as I can make out, the ovariotomy mortality is about 3·3 per cent.

I cannot find the exact dates of Keith's changes of practice, but before he used antiseptics he had 229 cases, with a mortality of 15·23 per cent.; with antiseptics he had 113 cases, with a mortality of 6·19 per cent.; and I believe his present results with clean water and frequent drainage are still better, but I have not seen any very recent record of them.

Bantock had 30 cases before he began antiseptics, with a 26·66 per cent. mortality; 6 cases with thymol, with 18·33 per cent. mortality; 87 cases Listerian, with 13·79 per cent. mortality; and then a series of 30 cases with such weak lotion that they were probably inert, with 13·33 per cent. mortality. He then gave up carbolic acid and the spray, but still prepared his sponges most carefully with a strong antiseptic sulphurous acid, and drained very frequently, clearing the tube at very short intervals, then he added the flushing-out process to his methods, and what with clean sponges, immediate removal of clot, &c., with pure water, and his method of frequent cleaning out of the drainage tube, he has adopted an aseptic method which has, I believe, yielded excellent results. Last year at this time he published 238 cases, with 21 deaths, or 8·8 per cent., but of the last 100 he only lost 4. Thus Bantock has a 4 per cent. mortality, and Tait over 3, against my 1·88, a small difference in favour of antiseptics it is true, but differences must be small when we come to single figure percentages.

Now let me very briefly sum up this far too long though very imperfect review of the past and present of abdominal surgery.

I hold that from the days of McDowell to 1876 or 1877, so little real progress was made in reducing the mortality of ovariotomy, that there was no encouragement to surgeons to attempt other abdominal operations; that with the abandonment of the clamp, there was a marked improvement, which was the chief cause of the introduction of other operations on the ovaries and tubes; the general use of the ligature was quickly followed by the general adoption of Lister's method, with such an immense advance in the success of ovariotomy, and the other operations on the ovaries, tubes, and uterus, that an impetus was at once given to the attempt to treat surgically many diseases of other organs in or partly covered by the peritoneum, which had up to this time been left to die under the care of the physician. Every operator of any eminence has improved his results enormously as soon as he adopted Listerism; then, having learnt how to be surgically clean, some have found for themselves ways of attaining this end with more or less success by methods differing from those of Lister. The drainage tube and the flushing with pure water both act in the same way: they remove the pabulum of the septic organisms. Lister, as Mr. Erichsen has recently well said, aims at keeping them out; Tait says this is impossible. I venture to doubt the correctness of Tait's view, because with a limited use of the drainage tube, and practically no use of the flushing, but with all the old means for keeping out the organisms, I obtain results which are, to say the least, as good as those obtained by any of the newer methods. My results are to some extent more valuable than those of any other operator, because they have been obtained with a steady adherence to one method, so that the value of the element of personal experience can be well seen. When a surgeon is constantly changing his method, it is impossible to say whether his results are improved by his increased experience, or by his change of method. The sum and substance of it all is, that if we had never had Lister to teach us true cleanliness, we should never have used antiseptics, flushings, or drainage tubes to attain it. The great advance is due to the antiseptic system; the minor details are merely the different ways of attaining the same end, asepticity. Time alone can show what is worth retaining, and what we may safely cast aside. Let us each then

follow our own method, aiming with single eye at the relief of suffering and the improvement of our noble science and art, and remembering the exhortation of Hufeland:—

"Thine is a high and holy office. See that thou exercise it purely; not for thine own advancement, not for thine own honour, but for the glory of God and the good of thy neighbour. Hereafter thou wilt have to give an account of it."

October 27th, 1891.

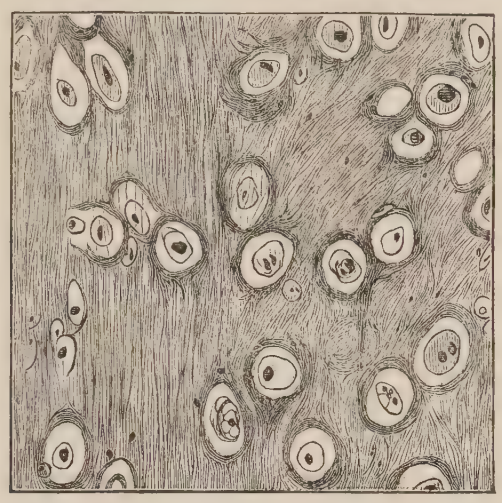
REMOVAL OF THE GASSERIAN GANGLION FOR SEVERE NEURALGIA.

By William Rose, F.R.C.S.

F. M., aged 60, was first seen by me on August 19th, 1888, in consultation with Mr. Padman. At that time she was suffering from severe neuralgia, affecting chiefly the inferior divisions of the fifth nerve on the right side of the face. This had resisted ordinary local and constitutional treatment. Stretching of the inferior dental nerve was decided on, and this was done on the following day, the patient being anæsthetised. The nerve was stretched just above its entrance into the dental foramen; it was also divided at its exit at the mental foramen, both steps being effected inside the mouth. This was followed by considerable relief until March, 1889, when the pain returned with great severity. On March 19th the lower jaw was trephined and a disc of the outer bony plate removed, thus exposing the nerve in the dental canal. Half an inch of the trunk was excised. Partial relief followed this, and it was not until March, 1890, that I was again consulted, the pain being more severe in the original situations, and in addition the right side of the tongue had become excessively painful, indicating an extension of the mischief to the lingual nerve. On March 18th, the inferior dental and lingual trunks were cut down upon in the pterygoid region through an incision parallel with the zygoma, dividing the masseter, and then deepening and enlarging the sigmoid notch with the trephine and

cutting pliers—an operation which I have performed on several previous occasions, and one originally suggested to me, and practised by, Mr. Victor Horsley. Portions of both lingual and dental nerves were thus excised. The result of this was to produce numbness and loss of sensation in the right side of the tongue and in the integument covering the right half of the lower jaw; but unfortunately the pain which had occasionally manifested itself in the upper jaw and cheek became greatly intensified. It implicated the alveolar border of the right upper maxilla, where the agony was greatest, and extended up into the temporal region to the top of the head. The slightest touch upon the gum produced a shock of agony that was terrible to witness, and the sudden approach of anyone or the banging of a door was sufficient to induce a paroxysm. Opiates and other sedatives had practically no effect. It was quite evident that the superior maxillary nerve was involved, and the patient's sufferings were so great that some serious mental derangement appeared imminent. Taking into consideration the previous relapses after partial nerve excision, I decided to make an attempt to remove the Gasserian ganglion, as nothing short of its destruction seemed to hold out any prospect of relief. I also decided to remove the superior maxilla at the same time, for the following reasons: First, on account of the extensive disease which probably existed in the nerves contained in it (and this was amply borne out by subsequent investigation); secondly, the increased facility thus given for opening the base of the skull; and thirdly, in deference to the express desire of the patient that, whatever else was done, the side of the jaw where the pain was greatest should be removed. Accordingly, on April 2nd, fifteen days after the last operation, assisted by Mr. Cheatle, surgical registrar to King's College Hospital, and Mr. Penny, house surgeon, the ordinary operation for removal of the superior maxilla was in the first place performed, and when the bleeding had been arrested and the foramen ovale exposed to view, the pin of a half-inch trephine was passed into the foramen, which acted as a centre, and the ring of bone surrounding it was carefully sawn and lifted out. The Gasserian ganglion could then be seen lying upon the apex of the petrous portion of the temporal bone, a small Schall electric illuminator being of great help at this stage of the operation. The ganglion was loosened by gently passing an aneurysm needle beneath it, and removed in three or four pieces by the aid of a narrow probe-pointed bistoury and fine-

hooked forceps, the dura being uninjured. The bleeding was slight. The incisions in the cheek were brought together in the



Drawing of a microscopic section of a portion of the excised ganglion, showing an increase of interstitial fibrous tissue between the ganglionic cells. Seen under one-fifth objective.

usual way with interrupted sutures of wire and catgut, cyanide gauze was applied, and the eye carefully protected with a soft pad. The patient suffered somewhat from shock, but her general condition on the following day was satisfactory. She, however, complained of heat and pain at the back of the right eyeball, which, on examination, showed considerable conjunctival congestion and chemosis. Next day this had increased, and was accompanied by haziness of the cornea, which subsequently ulcerated and eventually necessitated excision. In all other respects steady progress was made. The old pain ceased from the day of operation and has not since returned. She expresses herself as being in better health than she has been for years past. On October 25th she was carefully examined by Dr. Ferrier, and it was interesting to observe that, although sensation and taste were practically absent from the right anterior half of the tongue, they were distinctly present posteriorly. There was circumscribed anæsthesia of the right cheek and obvious wasting of the temporal, buccal, and other muscles on that side. The movements of the lower jaw were limited, probably on account of the operation in the pterygoid region. There was no paralysis of the facial nerve. The patient was shown to the Fellows of the Society, and also the ring of bone which was removed from the base of the great sphenoidal wing, together with the superior

maxilla, the infra-orbital canal of which was laid open, in order to demonstrate the thickened state of the nerves. Microscopic sections of the excised ganglion were also exhibited, together with diagrams showing the anatomy of the parts dealt with.

As far as I have been able to learn, this is the first instance in which the Gasserian ganglion has been successfully removed in the human subject. It is undoubtedly an operation of considerable danger and difficulty; at the same time I feel convinced that the severity of the symptoms and the hopelessness of being able to effect relief in any other way justified such a procedure. The loss of the eye is greatly to be regretted, and, it is to be hoped, need not necessarily occur in future cases. In the present instance the patient is more than contented with the immunity from pain which she now enjoys, and thankful to have gained it, even at the sacrifice of an eye. In any future case of the kind I should be inclined to stitch the eyelids together as an additional protection.

Dr. Ferrier congratulated Mr. Rose on the successful performance of a very formidable operation, which he believed was the first of its kind. He had seen it on one occasion attempted before, but the extirpation of the ganglion had not been possible, owing to the great hæmorrhage from the neighbouring sinuses. As the operation, however, was one which entailed considerable deformity, it was a question whether the relief from pain would compensate for this. On this point he agreed with Mr. Rose that it should be left to the patient to decide, and, considering the agony which many patients seem to experience, he believed that many would endure almost anything than the continuance of their sufferings. The extirpation of the Gasserian ganglion, or section of the nerve above this point, seemed to him to hold out reasonable hopes of permanent relief in such cases as the present; for, though the resection of one of the branches of the fifth nerve for inveterate tic was sometimes followed by apparently permanent relief, yet, in others, and often after long intervals, the pain was apt to return, either in the same region or in the course of the other branches. The pathology of neuralgia, as to whether it is peripheral or central, is in many points still obscure, but, whether the one or the other, there is reason to believe that the extirpation of the Gasserian ganglion would affect the whole nerve, and, perhaps, by ascending degenerative changes, alter the nutrition of the centres which might be the primary seat of irritation. As to the loss of the eye in this case, he believed that it was a pure accident, and one which need not result from the operation as such. As was well known, there were considerable differences of opinion as regards the so-called trophic influence of the fifth nerve on the eyeball, and many believe that the panophthalmitis which is apt to result from lesion of the fifth is due to the cutting off of the trophic influence of the Gasserian ganglion. He himself did not accept this doctrine as being at all satisfactorily proved, and held there were many facts which were

opposed to this doctrine. He was inclined to think that the affection of the eye was the result of irritative changes propagated along the nerve, and which need not always occur. It was as yet too soon to speak of the operation as an absolute success, so far, at least, as the relief of the neuralgia was concerned, for in many cases the pain was apt to return at an equally long period after resection of one of the branches. Dr. Ferrier thought it would be desirable if Mr. Rose were to report to the Society the condition of the patient at this time next year, and if, as he hoped, the patient were still free from her former terrible sufferings, then we should be justified in looking on the operation as the most valuable method of dealing with cases which proved intractable to all other methods of treatment.

Dr. Althaus congratulated Mr. Rose on the result of his operation, which was thus far very satisfactory, although it was questionable whether it would be permanent, and asked him whether he had removed the entire Gasserian ganglion. The very limited amount of anæsthesia which was now present in the skin and the mucous membranes of the face, as well as the absence of other symptoms which were observed in destruction of the fifth nerve, seemed to show that only a portion of the ganglion had been taken away. He referred to a case of disease of the entire fifth pair at the base of the brain, involving also the minor portion of the nerve, which had been under his care some time ago, and in which, apart from nearly complete anæsthesia, more especially great hyper-secretion of mucus in the eyes, nose, and mouth, and severe laceration of the tongue and the inside of the cheeks had been present, of which nothing was to be seen in Mr. Rose's patient. The loss of the eye in consequence of the operation appeared regrettable, and Dr. Althaus hoped that this might in future be avoided by giving great attention to the protection of the eyeball after such an operation, which Meissner had, by experiments on animals, shown to be most important. In his own case, although the disease involved the whole fifth pair, the eyes had not been lost; there had been keratitis and a limited amount of iritis, but no panophthalmitis, the optic nerves more particularly having remained healthy; and, in spite of a thick leucoma, which was present when first seen, the patient had eventually recovered a fair amount of vision, through clearing up of the cicatricial tissue under the influence of prolonged galvanic treatment. The complete relief of the agonising pain from which Mr. Rose's patient had suffered was a great achievement, and it would be very interesting to watch the further progress of the case.

Mr. Richard Davy criticised the operation from a purely surgical point of view. He congratulated Mr. William Rose on two points:—1. That he had saved his patient's life after a hazardous operation. 2. That for eight months after the operation his patient had been free from pain. Mr. Davy considered that the loss of the eye was not accidental, but a direct consequence of the operation, and that it would have been much better surgery to have limited the operation to removal of the superior maxilla (at any rate, for the time being). As the greater portion of the structures supplied by the first and second divisions of the fifth nerve had been lost, and paralysis of the masseteric group of muscles had followed this operation, Mr. Davy would not be inclined to submit to such an operation himself, or to repeat it upon a patient. The case was an illustration of "Diseases, desperate grown, by desperate appliance are relieved, or not at all." Mr. Davy thought that Mr. William Rose had in desperation moved one step further than preceding surgeons had, and that Mr.

Rose deserved credit for the very frank and careful way in which he had detailed the facts in this trying case.

Mr. Rose, in reply to the President, stated that, inasmuch as the previous operations had given such partial and temporary relief, he considered that mere removal of the upper jaw and the branches of the fifth nerve contained therein would not have been sufficient. Moreover, his previous experience in similar cases proved the inefficacy of such procedure. As regards the removal of the entire ganglion, his impression was that such had been accomplished, and this opinion was based not only upon the anæsthesia of the face, but also on the fact of the subsequent destruction of the eye, the upper or ophthalmic portion of the ganglion being the most difficult to reach. The area of anæsthesia on the face had certainly decreased, and that was probably due to fresh nerve anastomoses. Mr. Rose drew attention to the glazed appearance of the skin supervening shortly after the operation, and which was still evident, due, no doubt, to trophic disturbance. Considerable secretion was observed from the mouth and conjunctiva, both before and after the operation. He promised to inform the Society of the further progress of the case in twelve months.

Note.—July, 1891 (fifteen months after the operation). The patient's condition at this time is very satisfactory, and there has been no return of the neuralgia.

ON THE ABUSE OF THE DRAINAGE OF WOUNDS.

By C. B. Lockwood, F.R.C.S.

The systematic drainage of operation wounds was an advance in surgery which brought about such an obvious improvement in results that it became the custom for surgeons to use drainage tubes, or similar contrivances, with the greatest liberality. It would be easy, but hardly necessary, to support this statement with quotations from our works on surgery, and it is confirmed by what may be seen every day in our hospitals. I am well aware that of late there has been some reaction against indiscriminate drainage, and that the subject has been frequently mentioned in connection with the treatment of some of the larger joints, but, as it has not yet, so far as I can ascertain, been discussed by this Society, I venture to bring it forward this evening. I feel that I ought at the outset to apologise for alluding to facts which to many are quite familiar and elementary, and which are a part of their every-day knowledge, and also for introducing a certain amount of theory, which is very likely open to dispute.

It is inevitable that any great improvement in surgical practice

should at first be carried to extremes, and without doubt drainage was more needed when wound treatment was not so good as it is now; but each improvement in details has, I feel sure, made drainage less essential. I propose, therefore, to discuss the circumstances which seem to militate against a lavish resort to drainage, and to inquire into the conditions which demand or absolve its use. Let us begin with the question of its desirability in primary wounds made by the surgeon in healthy tissues, and upon patients presumably healthy.

Now, it will be readily admitted that, although drainage is in general favour, it has many disadvantages. An ideally successful wound is one which heals without constitutional disturbance under a single dressing. A drainage tube, or its equivalent, effectually prevents such a consummation. It not only (to refer to a self-evident fact) hinders union, but for its extraction the dressing must be taken off a few hours or days after the operation. Many will acknowledge that this is a serious misfortune to the patient, because the change of dressing is a painful and dreaded process, and attended with risks, especially as regards the subsequent asepticity of the wound, and the mere movement which it entails, as, for instance, that which cannot be avoided in dressing a stump, is most prejudicial to repair, and sometimes distressingly painful.

These disadvantages have been so clearly seen, that to avoid them soluble materials have been used for drainage by Nessler, MacEwen, and others; but these can hardly be said to have come into very general use, and, like other kinds, are open to the objection that they are very liable to become blocked. On this account I myself speedily discarded them, and of late I have only used other kinds with great reluctance, and in special cases, and with special reasons.

This brings me to one of the points in my argument, namely, the frequent inefficacy of drainage tubes. I do not know how many times I have found them blocked with blood clot, and the dressings hardly soiled. The frequency of this mishap is shown by two Syme's amputations, which I performed within a short time of one another. The first was done on a patient of Dr. Chill's, in whom gangrene was a complication of albuminuria. The wound had to be dressed for the first time on the fifth day for the sole purpose of taking out the drainage tube, which, after all,

was firmly closed with a tenacious clot. This wound was healed within the month, and without any constitutional disturbance. But the troubles which may attend a change of dressings were exemplified, because the perchloride lotion which was used at this dressing was stronger than the skin would bear, and in consequence there was some blistering, and a fresh dressing was needed in another week. The second Syme's amputation was done upon a patient sent to me by Mr. Girvin; the case was one of tubercular disease of the ankle joint with sinuses. Two of these latter were erased and irrigated and rubbed with iodoform, and tubes passed into them. The wound was dressed on the fifth day for the sole reason of removing these tubes, which, as in the first case, was firmly blocked with clot. The wound healed under the second dressing without constitutional disturbance. This second case indicated the chief condition under which blood clots are likely to render drainage tubes useless. When the dressings were changed, culture tubes of gelatine and agar-agar peptone were inoculated from the drainage tube and from the edges of the wound. These tubes of nutrient material remained sterile, and thus afforded strong evidence of the asepticity of the wound. Any permanent endurance of the clot would have been impossible in the presence of microbes, which, as is well known, have a powerful peptonising effect upon coagulated blood or coagulated serum. Therefore, I suppose we need not expect to see drainage tubes blocked with clot in septic wounds. Should such a thing occur, a speedy unblocking would inevitably ensue.

That broken down clots are septic was shown by one of my cases of amputation of the thigh. Some blood clot formed in the wound and broke down into the usual tarry fluid; and cultures showed that this contained abundant microbes (*Staphylococcus albus*). Here the drainage tube was useful and necessary, but only because the treatment had failed and resulted in a septic wound. Had the wound been aseptic, it may be assumed that the blood would have disappeared, as it may be seen to do in the chambers of the eye, or, as Sir Joseph Lister has described, have become organised and assisted in the process of repair. Perhaps I ought to add that the case which has just been mentioned made a rapid recovery; but, owing to the septicity, several dressings were required; the patient suffered some pain and discomfort; and unnecessary expense was entailed.

Perhaps the blood clots found in drainage tubes have not had sufficient attention given to them. The reason may be that blood clots shrink, and therefore those smaller ones which are often to be found in drainage tubes may have been much greater obstacles when they were younger.

But there is no reason to be anxious if a little blood does collect within a wound. There are plenty of facts in surgery to prove that the mere presence of blood amongst the tissues is not harmful. For example, there is great extravasation in simple comminated fractures, and in osteotomy wounds, and other subcutaneous injuries, but nothing happens. The tissues are always equal to the occasion, provided (and this is an essential point) the blood remains aseptic; and, if this be correct, a great deal depends upon the ability of the surgeon to achieve asepticism. Nor can I at present bring myself to agree with those who seem to think that a partial degree of asepticism is enough. No one will deny that many of the septic and infective diseases are due to organisms which may have a very wide distribution, and which may occur where we least expect them. Now, I know of no method of wound treatment which, whilst allowing the harmless to enter, can keep out the harmful. Therefore, to secure scientific certainty in the treatment of wounds all must be excluded—a thing more easily said than done.

But there is no necessity to consider blood an inevitable complication of wounds. When hæmorrhage is thoroughly arrested as soon as it occurs, and is quite stopped before the wound is closed, and when moderate elastic pressure is afterwards applied, none need be expected, and, in my opinion, no provision need be made for its escape. It is also to be noted that the immediate arrest of hæmorrhage the moment it occurs seems to make a difference in the healing. The tissues seem to soak up blood almost as readily as sponge does, and when thus infiltrated are clearly in a condition ill-fitted for repair or for resisting septic processes.

But to provide for the escape of blood is only one of the objects of drainage. Those who have seen wounds treated without drainage and with simple dressings must often, the day following the operation, have had the opportunity of seeing the flaps bulging with fluid. A director thrust between the lips of such a wound has permitted a discharge of blood-stained serum, which, if left behind, is thought, and upon good grounds, to be capable of

causing tension, surgical fever, and suppuration. Antiseptic dressings are often soaked with similar discharges, for the escape of which, I need hardly say, it is the main object of drainage to provide.

Now, if this is a truthful statement of the case, it is clear that, before we can do without drainage, two questions ought to be answered: first, is it correct to assume that these wound discharges always occur; and, secondly, is it correct to suppose that they are always harmful?

The conditions of the problem are such that it must always be hard, perhaps impossible, to answer the first of these questions decisively. Presently I shall allude to operation cases in which no discharges have been seen, but obviously it is always open to anyone to say that that is so because they have been absorbed. A few months ago I removed a very large lipoma from a woman's back, and a wound eight or nine inches long and almost as many wide was left behind. No drainage tube was used, but the wound was closed with precision and a reasonable amount of pressure applied with Martin's bandages. The dressing was removed on the tenth day, when the wound was healed without a trace of discharge. Culture tubes were inoculated with some of the sutures, but remained quite sterile, thus proving the wound aseptic. I am quite aware that this is not unusual, for wounds of this kind to heal in such a way as this one did, although antiseptics may not have been used, but I refer to it in preference to others, because its asepticity was tested and placed beyond doubt. Now, it is clear that in this instance there was either no discharge at all, or, if there was any, it was harmless in its nature, and easily absorbed. Indeed, the conditions under which repair took place must have been the same as in cases of simple fracture.

A similar absence of wound discharges was noted in a case of amputation of the breast with removal of the axillary glands. No drain was placed in the axillary wound, which was closed, and the axilla itself carefully packed. The dressings were removed on the tenth day to take out some button sutures, which had to be inserted into a part of the wound which could not otherwise be brought together. There was no discharge in this case and cultures inoculated from the wound remained sterile. The layer of gauze next to the wound was adherent and came off when the antiseptic dressings were finally removed on the seventeenth day.

As is always the case when the wound is proved to be aseptic, there was no constitutional disturbance. The absence of any obvious quantity of serous discharge from aseptic wounds is probably explained by the way in which they seem to heal. They repair without the usual signs of inflammation, and by a process so quiet and painless that it seems unjust to call it by the name of "inflammation."

Without doubt the elastic pressure which was applied to these wounds was an element in obtaining such an absence of discharge. The fluids in the smaller thin-walled vessels and in the interstices of the tissues are under a certain amount of pressure, comparable to and probably dependent upon the pressure of the blood in the larger vessels. It is obvious, therefore, that these fluids will tend to flow into areas of diminished pressure, as, for instance, into a wound, unless proper steps are taken to keep matters in a state of equilibrium.

Before proceeding with the second question, namely, whether the serous exudation is as harmful as is thought, perhaps I may be allowed a slight digression with regard to draining the axilla. I have gradually come to think that it is unnecessary, provided the wound is closed without contamination. Everyone knows how hard it is to keep the axilla aseptic for any length of time, but wounds in it are easily brought together and rapidly heal, at all events in their superficial parts. Now, I venture to argue, and my own experience bears out the argument, that advantage should be taken of this facility for repair, and instead of preventing the superficial parts healing by introducing foreign bodies, they should be permitted to do so before the axilla has had time to become septic. It is also obvious that the track of the drainage tube must remain as a place through which the deeper parts of the axilla may become infected.

A similar absence of discharge has been observed in various other undrained wounds, such as amputations of the fore-arm, hernia operations, and operations for the removal of tumours.

I have now tested a number of wounds by inoculating cultures from them, and whenever there has been an obvious amount of serous discharge the wound has proved to be septic. Perhaps it will be shown hereafter that wound infection is more potent than chemicals, foreign bodies, or want of pressure, in causing serous effusion, and that serous effusion cannot cause

suppuration unless it becomes septic. Now, inasmuch as it is practically impossible to say in any particular case that there has been no serous discharge, let us proceed with the second question, and inquire to what extent the serous discharge is harmful when it does occur. This question is the more important, because there are many wounds in which it is impossible by any adjustment of the dressings to avoid leaving spaces or irregularities in which fluids may collect.

Both observation and analogy show that *aseptic* fluids are harmless except that they may prevent union of surfaces by their interposition, or by dragging upon sutures. Nor can I conceive it possible for fluids which are not septic or chemically irritating to cause suppuration by producing tension. Let us note, in passing, that the word "tension" is, as Ogston says, exceedingly ambiguous. Is the pressure of the fluid in a hydrocele tension? Is the pressure of the fluid in an ovarian tumour tension? Is the pressure of fluid in a glaucomatous eye tension? Is the pressure of a ligature upon a pedicle tension? And yet how often are any of these accompanied with suppuration?

In a case in which I excised the head of the femur of a boy and closed the wound without drainage, and which has been recorded elsewhere,* it was obviously impossible to obliterate the acetabulum with pressure. I have no doubt that fluid collected, and some was seen on the dressing when it was finally removed. However, this case did uninterruptedly well and healed under one dressing. This was not, I believe, due to pressure having prevented exudation of fluid, but to the circumstance that the wound was aseptic. This was proved by the sterility of culture tubes which had been inoculated with sutures and with a little moisture from near the upper angle of the wound. And here I pause to remark that without such tests as these we cannot know what success attends our treatment. If tests were more frequently resorted to, many surgeons would, I venture to think, be obliged, after a short series of trials, to give up the idea that their wounds were aseptic. I further venture to think that they would also come to the conclusion that the aseptic wounds easily carry off the palm for absence of constitutional disturbance, rapidity of heating, painlessness, and economy. Further, when wounds have been

* 'Clin. Soc. Trans.,' 1890.

tested at the first dressing and found sterile, the greatest confidence has been felt that their subsequent progress would be safe and rapid, and that the first dressing would be the last. Of course I am speaking from a comparatively small number of cases, but as yet I have never been disappointed. It would be very interesting to hear what the experience of others has been in this respect. In schools where plenty of assistance is at hand it would be easy to carry out an investigation of this kind, and the results would be very valuable.

It would be easy to multiply instances in which serous discharge was absent, or in which it was harmless, but the preceding have been given because their asepticity was properly tested. They seem to show that, even in large wounds, drainage can safely and advantageously be dispensed with, but perhaps I may be allowed to give other instances to the same effect.

The treatment of wounds without drainage was put to a severe test in a case sent by Dr. Godfrey. The patient was a woman with very large breasts. An exploratory incision was made into the left to ascertain the nature of a growth, and this cut was extended and quite half the large-sized mammary gland removed through it. A deep and irregular cavity was left, and was closed without any drainage or dependent opening. The dressing was removed on the eleventh day, when, as the note says, the wound was healed, except at a corner where the skin had been badly brought together but which soon closed. This result was contrary to the anticipations of some who thought the omission of drainage exceedingly rash. I am sorry to say that owing to my absence the asepticity of this wound was never tested. Here, again, if any fluid was effused it must have been quite harmless.

In yet another case of amputation of the whole breast and the greater part of the pectoralis major, and a clearing out of the axilla, as much as possible of the wound was closed with button sutures and without any drainage. This wound was not touched for eight or ten days (I regret to say I have mislaid the notes giving the exact time when the button sutures were removed). Here, again, there was singularly little fluid of any sort, and, although the patient suffered severely from shock, there was hardly any constitutional disturbance. Indeed I cannot say that I have seen any ill-results follow from the omission of drainage except in the case of the scrotum, a point which will be mentioned later.

I do not for a moment maintain that drainage is a thing to be abandoned altogether, but that it should be made the exception and not the rule. In abdominal cases it is the rule not to drain unless there is a strong reason for so doing. Under ordinary circumstances reliance is placed in the absorptive powers of the peritoneum. Now I venture to argue that the absorptive power of the tissues when cleanly cut is not to be despised or ignored, and that it ought to have some trust given to it.

I confess that I have not always had the courage of my opinions and omitted drainage, and I have often had reason for regret. For instance, a few months ago I was obliged to amputate the thigh for senile gangrene. Before the operation the old man's temperature was 103°, and he was exceedingly ill. A drainage tube was introduced, and, although the patient had had a normal temperature since the operation, and was quite comfortable, the dressing had to be changed on the fifth day merely to take out this tube, which seemed to have been useless, to judge by the amount of discharge on the dressing. This wound healed with great speed under the second dressing. It may be added that cultures were inoculated from the interior of the drainage tube, but remained quite sterile.

I wish a greater number of cases treated without drainage and with very infrequent or permanent dressings could have been given; but the above are sufficient to show that the omission of drainage is not followed by such results as were seen before the practice came into vogue. To say the very least, the undrained wounds did as well as those which are drained, and not infrequently reached the ideal of perfection, by healing under a single dressing.

This small communication would be incomplete without referring to the conditions under which drainage seems still to be necessary.

There are some regions where it seems decidedly unsafe to operate without using it. The scrotum seems one of the worst, and I have come to the conclusion that if that region is encroached upon in hernia operation, or in the case of varicocele, or the like, it is almost essential to keep a tube in for the first forty-eight hours. The scrotum is so loose, its skin so difficult to clean, and it so impossible to compress, that the precaution cannot safely be dispensed with.

Wounds in the triangles of the neck are also, I feel sure, to be treated with caution. Here again we have to deal with a region in which fluids can easily extravasate and track towards places where their presence might be fatal. The neck, too, is in constant motion from respiration and deglutition, and it is obvious that pressure cannot be applied to it. Therefore I have always, when removing cancerous glands or tumours from any of the triangles of the neck, introduced a drainage tube for the first forty-eight hours. The same course would be advisable in the case of the ligature of arteries such as the carotid and perhaps subclavian, although it would be interesting to hear of successful cases done without, as we very likely shall. There may also be risk in leaving a cavity like that left after lumbar nephrectomy without a drain. It is also advisable to use drainage tubes in operations done upon tissues in which long-standing septic processes are established, such as chronic suppurative arthritis with old sinuses. But if cases of this kind are treated with every precaution it by no means follows that early or frequent dressings will be required. For example, I have recently amputated the thigh of a woman (sent me by my friend Mr. Arthur Cooper) for chronic suppurative arthritis of the knee of twenty years' standing. Although every attempt was made to do so, it was by no means certain whether an old and fœtid sinus had been thoroughly cleared, and therefore a small drain was introduced. The patient's temperature remained below normal and the stump was dressed on the tenth day. By this time there was a small quantity of fœtid discharge which had just come through—it smelt exactly the same as the old standing sinus did—but the stump was healed except where the tube had been, and the patient was able to move it about and hold it up in the air. The culture made from this wound contained a short bacillus and cocci (probably *Staphylococcus albus*) and it seems reasonable to suppose that the healing had taken place so rapidly in the parts of the stump away from the sinus that the newly formed reparative tissues were able to resist the septic process when it began at last to make headway.

With reference to cases which are septic, a distinction should be made between those in which the septic process is chronic and those in which it is acute. No one would demur to drainage in the acute septic processes, but it is not unlikely that those which

are chronic will ultimately, as our means of ensuring asepsis improve, be treated without. I have already instanced two chronically septic cases which illustrate this point. One was a Syme's amputation in which sinuses were made aseptic at the time of operation, and the other was a resection of the hip. The first, it may be remembered, was drained and its dressing changed on the fifth day; the second was not drained and healed under one dressing.

Lastly, the constitutional condition of the patient is to be taken into consideration. Infection of the wound by way of the circulation, or by auto-inoculation, as it is called, must be very rare. I do not know of any researches which throw light upon this question, but it must occasionally occur. I remember some time since having to cut down upon a median nerve which was engaged in some scar tissue. The wound did perfectly for the first week, and was practically healed, when the boy was seized with a diphtheritic throat, and his wound at once inflamed and suppurated. Doubtless it is wise to use drainage when obliged to operate upon people out of health.

Perhaps before this short communication is brought to a close the precautions which have been taken to procure asepticism ought to be mentioned. Wherever it has been possible the patient has had a bath the night before the operation, and the field of operation has been cleansed and prepared with scrupulous care. The hair has been closely shaved, the skin scrubbed with soft soap, and, if necessary, rubbed with turpentine, and finally bathed with perchloride lotion and put up in an alembroth dressing. The sponges and instruments, too, were prepared with scrupulous care; the former being treated, as our President, Mr. Thornton, directs for abdominal cases; and the pressure forceps and saws boiled for at least a quarter of an hour, and then soaked in 5 per cent. carbolic lotion, freshly prepared. Silk which has been boiled in 5 per cent. carbolic lotion and afterwards kept in the same has been used for both ligatures and sutures. Before closure the wounds have been irrigated with perchloride lotion 1 : 2000, and finally put up with 5 per cent. carbolic gauze well washed out in perchloride of mercury lotion next to the wound, and then alembroth wool, and a carbolic gauge outside dressing well adjusted with Martin's bandages.

Mr. Alban Doran commented on Mr. Lockwood's statement that "in abdominal section it is not usual to drain unless there be a strong reason

for so doing." "Strong" almost meant "extreme;" "a fair reason" would have been less misleading. Whenever free serous oozing followed the breaking down of adhesions (and this oozing was very frequent) it was best to introduce the drainage tube. This rule was especially important when the pelvic organs had been much handled by the operator. Drainage in abdominal section did not signify a contrivance by which fluid could run of itself out of the peritoneal cavity, but rather a means by which that fluid could be removed by the surgeon or nurse as it collected. The main precautions to be taken when the drainage-tube was introduced were three: Firstly, the fluid must be frequently removed by aid of a syringe—every three hours at the longest. Secondly, the dressings must not be arranged so as to press on the tube's mouth, else the lower end of the tube might slowly perforate the rectum. Lastly, it must not be left in too long. In the hands of those who know how to drain and when to drain, no practice had proved more salutary than the drainage tube in abdominal section. No doubt in amputations, &c., the tube was more of an active drain, and the duty of the surgeon was the removal of the effused fluid from the dressings into which it had already escaped out of the tube.

November 3rd, 1890.

AN OPERATION FOR VESICO-VAGINAL FISTULA THROUGH A SUPRA-PUBIC OPENING IN THE BLADDER.

By A. F. McGill, F.R.C.S.

An operation for vesico-vaginal fistula through a supra-pubic wound in the bladder was first suggested by Professor Trendelenberg. I have operated in this way twice. The first case was one of epithelioma, involving the floor of the urethra for its whole length, as well as part of the anterior vaginal wall and base of the bladder. The opening made in removing the growth was so large that, though I succeeded in bringing the edges of the hole which I had formed into apposition, the wound subsequently gaped, and a large vesico-vaginal fistula resulted. The chief interest in this case lies in the fact that in five weeks, without further treatment, owing to the efficient supra-pubic drainage, the fistula entirely and spontaneously closed. The second case was an ordinary one following labour, in which the operation was perfectly satisfactory.

Case 1.—E. S——, aged 50, was admitted into the Leeds Infirmary under my care on September 19th, 1889, suffering from retention of urine. For twenty-seven years she had taken laudanum (about an ounce and a half

daily). Her health was good, and she had no urinary symptoms till five months before admission ; she then noticed that she had difficulty in passing urine, that micturition was accompanied by pain, and that, occasionally, the urine was tinged with blood. The symptoms gradually increased in severity, till seven days previously she had complete retention ; she has not been able to pass any urine, but has been relieved by a dribbling overflow. On admission, the bladder was found to be much distended. There was a hard, nodular mass, which extended from the urinary meatus on to the anterior vaginal wall ; it involved the floor of the urethra and extended on to the base of the bladder. A female catheter was introduced with some difficulty, and several pints of urine were evacuated. The retention recurred. She was only relieved by frequent catheterism, and this caused much pain and some hæmorrhage. It was consequently determined to operate by removing the growth closing the vesico-vaginal fistula thus formed, and leaving a supra-pubic opening. On September 27th, the pubes having been shaved, the patient, who was a very thin woman, was placed in the inverted position by hanging her by her knees to a cross-bar attached to an operating table, and supporting her loins by pillows. Her head was placed towards a window, the full light of which, consequently, fell on the abdomen. The bladder was now distended with 12 ounces of a boracic acid solution. A transverse incision, about half an inch above the pubes, and 3 inches in length, was made ; this divided both skin and recti muscles. The bladder was exposed, fixed by a tenaculum, and opened transversely ; it was fixed to the deep part of the abdominal wall, and thus prevented from falling into the pelvis, by three sutures. The patient was next placed in the lithotomy position, and a duckbill speculum introduced into the vagina. The growth, pressed down by the fingers of an assistant passed through the supra-pubic wound, was removed by scalpel and scissors. It was more extensive than was expected. The lower two-thirds of the circumference of the urethra, as well as part of the base of the bladder, was implicated, and, when the removal was complete, an opening, which would easily admit two fingers, had been made. This opening was closed by fine silk sutures passed through the vaginal wall. The edges were with difficulty brought together, and there was much tension on the stitches. The patient was now replaced in the inverted position, and the opening in the mucous membrane of the bladder was closed by fine catgut sutures. Lastly, the supra-pubic wound in the bladder was closed at each end, an opening being left large enough to admit a drainage tube ; the recti muscles were stitched together, and the skin wound closed except at the centre. On injecting the bladder, it was found that the opening into the vagina was watertight. The pathological report made it certain that the growth was a squamous epithelioma. The subsequent history was as follows :— On examination at the end of a week, it was found that the posterior part of the bladder wound had not united, and that a vesico-vaginal fistula, through which a finger could be passed, existed. The supra-pubic wound closed slowly, and the opening into the bladder was kept patent by a tube introduced at intervals. The bladder and vagina were irrigated twice daily through the supra-pubic opening with boracic solution. On November 3rd, thirty-seven days after the operation, the opening between the bladder and vagina was completely healed, and all urine escaped by the supra-pubic wound. The patient was fitted with a urinal, and sent to our convalescent home. On January 4th, 1890, she returned to the infirmary, when it was found that urine still escaped by the side of the urinal tube. There was no recurrence of the growth. On January 18th she was sent home.

Case 2.—E. A. W——, aged 17, was admitted into the Leeds Infirmary under the care of Dr. Braithwaite on January 3rd, 1890. Knowing that I was interested in the subject, Dr. Braithwaite kindly transferred her to me. She was delivered by forceps on November 23rd, 1889, after having been forty-eight hours in labour. Since then all urine has passed by the vagina. On examination the perineum was found to be lacerated to the anus. There was a vesico-vaginal fistula, large enough to admit the tip of the index-finger, situated immediately in front of the os uteri. On January 11th the patient was placed in the inverted position, and the bladder opened transversely and fixed to the abdominal wall, as described in the previous case. It was necessarily impossible to distend the bladder. A copper spatula was introduced into the bladder, and its superior wall pressed upwards. A small electric light was placed in the bladder against the spatula, and the whole of its interior and the fistula were perfectly illuminated. The fistula being fixed and pressed upwards by two fingers of an assistant introduced into the vagina, the edges were refreshed with great facility. Four chromicised catgut sutures were passed through the mucous membrane, and the fistula thus completely closed. As a further safeguard, the patient was placed in the lithotomy position, and four fine silk sutures were inserted, including all tissues except the bladder mucous membrane. The supra-pubic wound was closed in three layers, bladder, muscles, and skin, an opening for a drainage tube being left in the middle line. The after-progress was in every way satisfactory. On the fifth day the tube was removed; on the eighth she passed a little urine by the urethra, and at the end of a fortnight the vaginal sutures were removed. In less than a month the supra-pubic wound was completely healed, and on February 13th she returned home, passing urine naturally, and perfectly restored.

The operation which I have described above may, I think, be somewhat modified with advantage. I suggest that it be practised as follows:—1. The patient is placed in the inverted position.* As the bladder cannot be distended, and as the rectal bag would be of little, if any, use, it is advisable that the peritoneum be removed as far as possible from the pubes. This is best effected by the position adopted. In this position a side light best illuminates the interior of the bladder, or an electric light can be used with most advantage, and thus the subsequent steps of the operation can be carried out with ease and facility. 2. In the two cases which I have described I made, as Trendelenberg advises, a transverse incision. In future I intend making a longitudinal incision through the skin and linea alba, and opening the bladder in the same direction. If necessary, it will be easy to partially divide

* The patient is easily inverted by means of a T-shaped support attached firmly to the end of an operating table. The height of the support can be changed by a crank which works the upright. We use Verity's patent epicycloidal window holder, which is firm and self-fixing, for this purpose, and find it convenient. In private practice the patient can be inverted over the top of an ordinary small iron bedstead.

the recti, and thus obtain more room. It is true that with this longitudinal incision the view obtained of the interior of the bladder is not quite so good, and that manipulation in its interior is not so easy. On the other hand, my experience of eleven cases of transverse supra-pubic cystotomy has shown me that healing is, as a rule, much slower with a transverse than with a longitudinal incision. In cases like the one first related, where a supra-pubic urinal must be worn, the transverse incision is certainly inadmissible. The recti, if undivided, form a sort of sphincter and grip the tube introduced into the bladder, thus preventing the escape of urine by its side. If these muscles are divided urine will escape by the side of the tube, thus making the urinal comparatively useless. These remarks apply not only to cases of vesico-vaginal fistula, but to all cases of supra-pubic cystotomy. 3. The bladder must be fixed by sutures to the deeper layers of the abdominal wall. These are easily introduced by large corkscrew needles in handles. The long ends of the sutures are not removed till the operation is finished. They prevent the bladder from being displaced from the abdominal wall, and they serve the purpose of retractors, opening the wound and facilitating a view of its interior. If expedient, a metal spatula may be introduced and used as a retractor with advantage. 4. The edges of the fistula are refreshed, care being taken to remove a complete ring of vesical mucous membrane, and of the bladder and vaginal walls. 5. Fine catgut sutures are passed through the mucous membrane from above, and the patient being placed in the lithotomy position, silk sutures are introduced through the vaginal wall from below. In this way the fistula is completely closed by a double row of stitches. 6. The supra-pubic wound in the bladder and in the abdominal wall is partially closed, room being left in the middle line for the introduction of a large rubber tube. Supra-pubic drainage must be maintained till the fistula is firmly closed. It is this drainage which is the important factor in this operation. As the bladder cannot become distended, there will be no tension on the sutures which bring the edges of the fistula into apposition, and no tendency for the urine to become infiltrated into the wound. The cystitis which is apt to occur when a catheter is frequently passed or tied in the bladder will not be likely to occur, and, if necessary, the cavity of the bladder can be kept clean by free irrigation with a weak antiseptic solution. It may, consequently, be expected that in most

cases the operation for the relief of vesico-vaginal fistula following labour will be successful at the first attempt. Case 1 makes me hope that cure may result even in the worst cases, for it shows that a large fistula will close without further treatment if the urine can escape freely through a supra-pubic opening.

In conclusion, it may be asked, Is there any need for this operation? My experience may be unfortunate, but I have seldom seen a vesico-vaginal fistula closed at one sitting. A small opening is not unfrequently left, and the discomfort of the patient is the same, whether the opening is small or large. Not unfrequently the operation must be repeated several times before a perfect result is obtained. I have seen other cases in which repeated operation has failed to give relief. When this occurs the supra-pubic operation should, undoubtedly, be tried. My small experience does not warrant me in expressing an opinion as to whether it should be adopted in more favourable cases.

Dr. Routh thought Mr. McGill's paper would form an era in the treatment of bad cases of vesico-vaginal fistulæ. He must say Mr. Thornton's success was much higher than that he had seen to follow other operators. Mr. Baker Brown, whose many operations he had seen, and in which he had assisted him, besides those he had operated upon himself, was considered about the best operator of his day, but he, Dr. Routh, knew Mr. Brown had sometimes to operate several times in the same case—in one seven times, in another thirteen times—before a cure was completed. In must be so in those cases where the rent made was very extensive, sometimes involving the whole inferior floor of the bladder, or in cases long neglected with *hard cicatrices*. The plan of *splitting* the vesico-vaginal membrane was not altogether new. It had been practised twice, in two cases, the mucous membrane of the bladder being ligatured first, and then that of the vagina, including the muscular tissue afterwards, somewhat after the same way as was done now in cases of ruptured perineum. Vesico-vaginal fistulæ were mainly due to women being kept in labour for days before delivered. Hence in the backwoods of America they were common, also in China, where nothing was attempted in delivery till four days after labour, and where through sloughing the openings were very large. Such cases were few in England, and became fewer every day, owing to the surfeit of medical men, but where they did occur Mr. McGill's method would cure where no other method would, and therefore deserved the greatest commendation.

Mr. Edmund Owen remarked that the good and original work which Mr. McGill had already accomplished in connection with the surgery of the bladder would of itself ensure attention to, and respect for, the suggestions which he had just made in connection with the treatment of vesico-vaginal fistula. Nevertheless, he (Mr. Owen) could not but think that the value of the paper was somewhat diminished by the publication with it, as an illustrative case, of the report, No. 2, in the Appendix. It was that of a simple vesico-vaginal fistula, such as the surgeon might very reasonably expect to close by a single and simple operation through the

vagina. That the fact of the perineum being extensively ruptured made the fistula the more readily accessible, and its effectual closure an easy matter, and that, probably, without the aid of artificial light. That henceforth, however, the surgeon would know that for intractable or unpromising cases supra-pubic cystotomy might with advantage be resorted to, and, apparently, without incurring any considerable risk. That for Mr. McGill's paper the Medical Society and operating surgeons generally were greatly indebted.

Dr. Collingworth considered that the operation described in Mr. McGill's paper furnished a valuable addition to our resources in dealing with rebellious cases of vesico-vaginal fistula. The individual experience of surgeons in this country, with regard to this operation, was limited, owing to the fact that British midwifery practitioners were alive to the evils of leaving the head of the child too long in the pelvis. But the conditions in other countries were not always so favourable. Probably the largest personal experience that had fallen to the lot of any living British surgeon was that of Mr. Milton, of the Civil Hospital at Cairo. In the 'St. Thomas's Hospital Reports,' vol. xvii, for 1887, Mr. Milton had placed on record fifty cases in which he had operated for vesico-vaginal fistula and ten for recto-vaginal fistula. The paper was one of great value, and it was a great pleasure to him (Dr. Collingworth) to have an opportunity of directing attention to it. Different methods of operating were employed, and different methods of conducting the after-treatment, the results being carefully analysed and compared. A considerable number of the cases required a second operation, and some were operated upon a third and even a fourth time. Even then, no fewer than twelve cases remained uncured, a percentage of 24. We might conclude from this that there must always be a residuum of cases where the ordinary methods of operating failed to effect a cure. In these cases, Trendelenberg's operation offered an additional resource, and Mr. McGill certainly deserved the thanks of the Society for having introduced it to their notice in the able paper to which they had just listened.

Mr. McGill agreed with the previous speakers in thinking that the operation would be more generally used in severe than in slight cases. In cases that could not be closed at one sitting, he advised a trial of the supra-pubic operation. He regretted that he had been unable to give the history of his first case to the present time. There was no return of the epithelioma four months after removal, but he had been unable to further trace the woman.

A CASE OF PELIOSIS RHEUMATICA.

By Thomas Whipham, M.D.

Agnes J——, aged 17, was admitted into St. George's Hospital under my care on October 12th, 1889.

She was a general servant who, according to her mother's statements, was neither overworked nor underfed. Both her parents were living, and she had three brothers and four sisters, all in good health. A paternal uncle died of inflammation of the lungs, and an uncle on the mother's side of consumption. There was no

history of rheumatism, chorea, or diseases accounted hereditary in the family, with the single exception of the instance of consumption above mentioned. The patient herself had measles and whooping cough as a child, and at the age of 3 had so bad an attack of inflammation of the lungs that (so her mother said) she was given up for three weeks. She had been liable to attacks of "bronchitis" for some time. Otherwise she appeared to have enjoyed good health.

She was first taken ill on October 9th, 1889, with pain in her limbs and back, chilliness, and vomiting, but nevertheless she managed to take some children out for exercise on the following day.

On October 11th she was very weak, and complained of pain in her knees and sore throat. She then left her place as a servant and went home.

On October 12th she was admitted into hospital, noisily delirious, evidently with pain in both knees and effusion into the joints, and at once put on sod. salicylat. ʒss., potass. bicarb. ʒj, with some spirit of chloroform every four hours, a draught of salts and senna having been previously administered. It was further ordered that she should be sponged with cold vinegar and water every hour.

On October 13th she vomited after the medicine, and was delirious in the afternoon, but even when the delirium had subsided during the following morning it was not possible to obtain from her any account of her illness or her present feelings. At the base of each lung some crepitations were audible. The fauces, uvula, and pharynx were red and rather dry, and some white patches were adherent to the soft palate. At 9.0 A.M., the temperature having fallen from 105° F. on admission to 101° F., the house physician stopped the sponging. Some prussic acid was added to the mixture with the view of checking the vomiting.

October 14th.—She had a tongue somewhat dry, and in appearance resembling that usually seen in fever, *i.e.*, a red tip and edges, a dry brown central longitudinal streak, and on either side of it a thick white fur. Her skin was dry and harsh to the touch. Percussion and auscultation revealed nothing amiss at the back of the lungs, but in front of the left lung some crackling râles were audible. The patient was continuously more or less delirious. Over the apex of the heart a soft systolic murmur was heard. There was still effusion into the knee joints, but certainly less than when she first came under observation; apparently there was no pain in these joints, but it was difficult to be sure of this. As her

pulse was failing, five ounces of brandy were ordered. The urine was pale, 1·030, acid, with a deposit of urates, and a thick cloud of albumen was thrown down by nitric acid. In the afternoon, the temperature having fallen to 99° F., the salicylate was omitted and four additional ounces of brandy were ordered; and at night a draught of chloral hydrate (ʒj) and ammonium bromide (ʒss).

October 15th.—She had a much quieter night, but this morning was slightly delirious at times; her aspect was heavy, and her complexion dusky. The tongue had now a universal white, creamy coat, such as is seen in rheumatism. The abdomen was distended, no eruption and no apparent tenderness. A few crackling râles were heard over the left lung anteriorly. The heart's action was rather forcible, and a faint systolic murmur was audible at the base chiefly. The effusion into the knee joints remained. The night draught was repeated.

October 16th.—She slept after the draught for ten minutes only. Her expression was very anxious; her lips were dry; her tongue was dry. The abdomen was still distended, and the liver could be felt two finger-breadths below the ribs; the spleen was not obviously enlarged; she was less delirious and complained of pain in her neck. There was less effusion into the knee joints, which were neither tender nor painful. The cardiac murmur was systolic, and most marked over the the pulmonary artery. Fine râles, chiefly with inspiration, were heard over the front and base of the left lung, rather sharp and crackling in character, and there was a little increase of voice sound on the right side, over the upper part of the lung. In the morning a profuse blotchy eruption (erythema multiforme) was observed on the backs of the hands, which in the evening had extended to the arms, legs, and face. The eruption bore a great resemblance to that of measles.

On October 17th the eruption had disappeared from the body, but remained on the forearms, knees, and feet. Fine crepitant râles were audible all over the left chest, and loud rhonchi on both sides.

Three grains of quinine were ordered every four hours, and the patient was put on sp. vin. gall. ʒviij daily. The respirations were very rapid. The heart sounds were inaudible, in consequence of the râles and rhonchi accompanying respiration.

The abdomen was much distended with flatus, in spite of which percussion showed enlargement of the spleen; the tongue was coated with a white, creamy fur; the lips were dry and dusky; the

pupils equal, but sluggish in responding to light. The pulse was running; the patient very restless, crying out in her delirium. She died early next morning, a muttering delirium persistent till death.

Autopsy, 30½ *hours after death*:—

Save in the hands and feet, rigor mortis was slight. The dependent parts of the body were very livid, and especially so the face and neck. On the shoulder and arms were many crescentic pinkish-brown maculæ; the skin over the patches was not raised. The knee joints contained a considerable excess of clear synovial fluid; the cartilages were quite natural in appearance. The pleural cavities were almost obliterated by old adhesions; at the back and lower parts of both was a little clear fluid.

Both lungs were congested and œdematous, less expanded than normal; the whole of the lower lobe of the left was almost black, and so far infiltrated with blood that it was devoid of air, and sank in water.

The thyroid body was somewhat enlarged, by reason of an increase of its colloid constituents and congestion.

The cavity of the pericardium was obliterated by the formation of extensive adhesions, which could be broken through with moderate force. In the meshes of these adhesions, which were of a light yellow colour, extensive extravasation of blood had occurred. Beneath the pericardium the heart muscle was in a state of chronic interstitial inflammation. The patches of interstitial myocarditis extended to different depths in the substance of the left ventricle; the inner surface of the myocardium showed in a marked degree the "thrush's breast" appearance of fatty degeneration.

The liver was in a state of cloudy swelling, passing to fatty degeneration. Its weight was 48 oz.

The spleen weighed 8 oz., was quite firm and natural in colour.

The kidneys were congested. On the mucous coat of the stomach were a few congested patches, and here and there ecchymosis. Patches of congestion were also found in the duodenum and ileum, such as might have been produced by some irritant; others of the same nature were observed in the cæcum and colon.

There was slight endometritis.

The dura mater was adherent to the skull; there was slight congestion of the brain generally, but it was firm, and otherwise natural.

This case is submitted to the Society as one of much interest and some obscurity. It has been considered, after due estimation of its characters, symptoms, and appearances, to be an instance of rheumatism, although many of its symptoms differ widely from those usually observed in that disease.

The onset of the attack, with its pain in the back and limbs, its chilliness and vomiting, resembled rather the commencement of one of the eruptive fevers than that of an ordinary attack of acute rheumatism. On the other hand, the occurrence of sore throat and pain and effusion in both knees, which came on two days later, were indicative of the latter disease, while the absence of infarcts or circumscribed areæ of congestion negatived the probability of pyæmia.

The appearance of the tongue served only to increase the difficulty of the diagnosis; for on the third day after the patient's admission into hospital it had the appearance, as has been stated, of the tongue usually seen in fever. On the following day it was covered entirely with the white, creamy coat characteristic of acute rheumatism.

On the assumption that the case was really one of rheumatism, sodium salicylate with potassium bicarbonate was ordered every four hours, with the result that the temperature fell from 105·2° F. on October 12 to 100° F. on the morning of October 14. It then, in spite of the drug, rose to 104° F. in the evening of that day, until, with slight intermissions, it again reached 105° F. on the day before the patient's death, the seventh of the disease.

Judging by this temperature record, in a person taking what may be considered an ample dose of the sodium salicylate, the case was clinically unlike acute rheumatism, as ordinarily observed. At the same time it can hardly be regarded as an instance of hyperpyrexia.

Still the pain and serous effusion into the knee joints, which showed a decided tendency towards absorption, in spite of the general intensity of the fever, the eruption of erythema multiforme, not uncommon in rheumatism, and the post-mortem evidence of pericarditis, all point to the rheumatic nature of the disease. Furthermore, the firm, and, but for its enlargement, healthy, condition of the spleen may be taken as evidence against the disease being one of the ordinary continued fevers or pyæmia.

The appearance of a white film on the soft palate, with the

Agnes J.

MONTH	*October, 1889.*													MONTH
DAY	*12*	*13*	*14*	*15*	*16*	*17*								DAY
F°	M E	M E	M E	M E	M E	M E	M E	M E	M E	M E	M E	M E	M E	C°
107°														
106°														41°
105°														
104°														40°
103°														
102°														39°
101°														
100°														38°
99°														
98°														37°
97°														
96°														36°
M. PULSE		*126*	*114*	*112*	*120*	*150*								M. PULSE
E.	*123*	*116*	*102*	*106*	*144*	*144*								E
M. RESP.ⁿ		*30*	*30*	*26*	*38*	*42*								M. RESP.ⁿ
E.	*34*	*30*	*30*	*26*	*46*	*48*								E
DAY OF DISEASE	*3*	*4*	*5*	*6*	*7*	*8*								DAY OF DISEASE

HARRISON & SONS, LITH. ST. MARTINS LANE, W.C.

presence of albumen in the urine, should receive word of notice as being suggestive of diphtheria. Subsequent observations showed that in all probability this film was due to the adhesion to the mouth of some viscid saliva mixed with milk, and that it was not a diphtheritic membrane.

Reviewing the facts and symptoms, it seems probable that the case was really one of rheumatism in which an external eruption (erythema multiforme) occurred, and that the case was further complicated by hæmorrhage or purpura in the lungs, in the adhesions developed between the two layers of the pericardium, and in the mucous coat of the stomach.

On reference to the tables on rheumatism recently published by the Collective Investigation Committee of the British Medical Association, it is found that, out of the 655 cases tabulated, purpura is recorded in a few, viz., 0·76 per cent., four males and one female; of these five two died, one male, aged 34, and one female, aged 22; both were of intemperate habits. The latter, in addition to very extensive purpura all over the body, also suffered from epistaxis; the former had purpura from head to foot from the second day of the disease until the fifth day, or two days before his death. The remaining three cases were males, aged 15, 18, and 26, of which two were mild attacks of rheumatism, while the third, a labourer of 18, temperate in his habits, was severe.

Further examination of these tables as to the frequency of erythematous eruptions in acute rheumatism gives the following results:—

Erythema occurred in 1·37 per cent., viz., in seven males and two females, of whom eight were temperate, and one, a married woman, was a teetotaller. The youngest was a lad of 13; the oldest a woman, aged 44. In five cases the attack of rheumatism was severe; in four it was of moderate severity; and one, a gardener, of temperate habits, was a case of hyperpyrexia, which ended fatally.

Erythema multiforme occurred in 0·30 per cent., viz., two males, aged 13 and 17. In both the rheumatic attack was of moderate severity. Roseola is recorded in 0·30 per cent., viz., two males, aged 24 and 17, the one temperate, the other a teetotaller; in the former the attack was severe; in the latter a moderate one.

The above figures are given to show that purpura and various erythematous eruptions, though by no means unfrequent accom-

paniments of rheumatic fever, occur in but a small proportion of the cases; and that, as in many other diseases, purpura as a complication of rheumatism, more especially in those who have become debilitated by excesses, &c., is of grave import.

In the volumes of the 'Transactions' of four of the societies of London (Medical and Chirurgical, Medical, Pathological, and Clinical), I find but one case of hæmorrhage as a complication of acute rheumatism. It is recorded in the 'Clinical Society's Transactions,' vol. ix, p. 74, by Mr. H. G. Trend, as an instance of purpura rheumatica, and it is so remarkable a case, that perhaps an epitome of it may not be out of place on the present occasion. The patient was a lad of 17, who was occasionally dyspeptic and weak, and who had recently become "thin, anæmic, and altogether below par." While engaged one day in cellar work, a cork flew out of a bottle, and inflicted a deep wound on his right forearm. The wound was very slow to heal. Some six weeks after the accident, the boy became feverish, and had an attack of tonsillitis, followed a few days later by a rigor and swelling of the left elbow; at the same time both ankle joints were very tender. On the day following the rigor some purpuric spots appeared on the left elbow; the whole arm rapidly swelled, became cold, and was "literally as black as a coal from the shoulder to the tips of the fingers;" eventually some purpuric spots appeared on the left ankle. The urine was free from albumen. A few days later "a bloody stool was passed," and purpura was observed on the left thigh and leg. Finally, a slough formed on the left forearm, and the patient made a good recovery. The boy was regular in his habits, and in easy circumstances, but had a great objection to vegetables. This case is an interesting one, if only for the fact which is recorded concerning it, viz., that Sir William Jenner and Dr. Russell Reynolds saw the patient, and were unable to account for the hæmorrhagic complication. It may be objected that it is not quite on all fours with my own, but still I think that the resemblance between them is a pretty close one. It is true that my patient had no external hæmorrhage, and that Mr. Trend's had but one symptom of internal bleeding, viz., the bloody stool, but this is scarcely sufficient difference to necessitate a distinction between the two. For my own part I am inclined to consider them both as examples of peliosis rheumatica.

On reference to the records of St. George's Hospital for the

last ten years, it is found that there have been twenty-two fatal cases of acute rheumatism and its immediate complications. In these hæmorrhage has occurred in some form in 22·72 per cent., viz. :—

1. A child of 7, in whom epistaxis was profuse.
2. A male, age 22, who had purpura.
3. A male, aged 22, into whose pericardium extravasation of blood had occurred.
4. A male, aged 24, in whom extravasation of blood was found into the pleuræ, lungs, intestines, and skin.
5. A male, aged 23, in whose stomach and intestines large quantities of altered blood were found, there being no ulceration of the mucous membrane.

That purpura occurs in rheumatism, as in other diseases and conditions of the body, both in cases which recover and in cases which are fatal, is well known. Such an extensive extravasation of blood as is recorded in the present instance is of sufficient rarity as to be worthy of the notice of the Medical Society.

In reply to various speakers, Dr. WHIPHAM remarked that he entirely agreed with Dr. Gilbart Smith as to the difficulty in the diagnosis in this case. It was impossible to say definitely that it was not an instance of pyæmia, but at the same time many of the points in the clinical history and the post-mortem appearance were rather averse to such a conclusion. On the other hand, the evidence of comparatively recent pericarditis, the serous effusion into the knee joints, which had been partially absorbed before death, and the healthy condition of the cartilages, &c., of the knee joints at the autopsy, indicated rather a rheumatic origin of the symptoms and lesions. He had hitherto no experience of cases in which such hæmorrhage had attended the administration of sodium salicylate, although he acknowledged the possibility of an idiosyncrasy with regard to this drug, such as is occasionally met with in the case of potassium iodide.

November 10*th*, 1890.

THE RELATION OF ASTHMA TO OTHER DISEASES.

By SAMUEL WEST, M.D., F.R.C.P.

IN speaking of *Asthma*, I must premise that I use the term in its strict sense, and that I do not refer to those cases which are

often erroneously called asthma, but which are really of an entirely different nature.

If I must begin with a definition, I should say that asthma consisted in a paroxysm, or a series of paroxysms, of peculiar dyspnœa, usually recurrent, and often periodic, commencing suddenly, rapidly reaching great intensity, accompanied with loud wheezing and defective respiratory movements, both of inspiration and expiration, but chiefly of the latter, usually with depression of the diaphragm so that the lungs are distended to their maximum, lasting a few hours and terminating somewhat less suddenly than it began, not necessarily attended with either cough or expectoration, but commonly ending with more or less of mucous expectoration, unattended by fever, not associated with any constant organic lesion, and compatible with good health when the attacks are past.

This, however, it is evident, is rather a description than a definition, and is a statement of facts and not of theory: I desire in what is to follow to keep strictly to facts, to state what is known and has been observed of asthma in relation to other affections. I shall then consider the conclusions to which these facts appear to lead.

1. *Affections of the lungs* naturally offer themselves first of all for our consideration, but it may be stated at once that asthma stands in no definite relation to any disease of the lungs, except that if it be long continued it is likely to lead to permanent emphysema and chronic bronchitis. In childhood it is often started by an attack of bronchitis or measles, and the same may happen in the adult. With acute affections of either the lung or pleura, the liability to asthma usually disappears for the time, to return when convalescence is established. Phthisical patients very rarely suffer from spasmodic asthma, and if an asthmatic patient becomes phthisical, an event which is by no means common, the asthma usually disappears. This occurred in the two following cases: (1) A man, aged 32, had suffered from asthma for four years when first seen. The attacks were typical and no physical signs of phthisis existed, but four months later he spat a little blood, and eight months later had definite signs of a cavity at the right apex, the asthma having been absent for many months; (2) a female, aged 36, had been asthmatic for nine years; the attacks were preceded by pain under the right nipple, and they were often

followed by epistaxis. Some months later she was seen with well-marked phthisis at the right apex, but the asthma had ceased.

2. *Diseases of the Heart.*—More or less of permanent dyspnœa is the rule with all forms of *morbus cordis*, and paroxysmal attacks are by no means rare. The paroxysms may be due to sudden failure of the heart, to acute œdema of the lung, to infarction or embolism, but they differ entirely from an asthmatic paroxysm. True cardiac asthma is extremely rare, perhaps the rarest of all forms, as it is also the most difficult to treat and the most doubtful of prognosis.

Cases have been described under the name of cardiac asthma in which the pulse has been very rapid, over 200 in the minute, but without excessive dyspnœa. From the accounts given they appear to me to be not cases of asthma at all, but of that functional disease of the heart which I have described under the title of Paroxysmal Hurry. To angina pectoris I do not think asthma has any real relation. I have at any rate not seen any case which would suggest a connection between the two affections.

3. *Affections of the Stomach.*—Asthma is very rarely found in connection with any organic disease of the stomach, but rather with functional derangement only; and as in most instances the paroxysm does not come on until an hour or two after a meal, it is due probably rather to the circulation in the blood of some irritating product of imperfect digestion than to any actual irritation in the stomach itself.

Henoch* gives two cases in children in which the attacks were immediately cut short by vomiting, in the one spontaneously and in the other after a mechanical emetic, and he refers to another which was cured by creasote. Even intestinal dyspepsia may be associated with asthma, which may be relieved by free evacuation. Perhaps in this connection may be mentioned those cases which have been referred to the presence of worms (*asthma verminosum*).

4. *Affections of the Kidney.*—Paroxysms of dyspnœa again are by no means uncommon in these affections, and may be due to many causes—to failure of a hypertrophied heart, to acute or chronic œdema of the lung, or to uræmia: but true asthma is rare. In uræmia, respiratory disturbances of paroxysmal character are

* 'Berlin. klin. Woch.,' xiii, 18, 1876.

frequent, sometimes with dyspnœa and sometimes without, as in Cheyne-Stokes breathing. In most cases the attacks bear a closer relationship to those which occur in *diabetes mellitus* than in asthma, and depend probably, like them, upon some poisonous substance circulating in the blood. At any rate true spasmodic asthma is rare in renal disease of any kind.

We may conveniently associate with renal disease those two affections which are so commonly connected with it, namely, gout and lead-poisoning, to both of which asthma has occasionally been referred.

5. *Gout.*—Trousseau* was the first, I believe, to draw attention to this association, and recorded a remarkable case in a boy of 5 years of age who, after suffering from asthma for two years, developed gout in his big toe. In another case of long-standing asthma, gout developed at the age of 21, after which the asthma never returned, but the patient became a martyr to hemicrania.

6. *Rheumatic Fever.*—Trousseau also records a case in which rheumatic fever commenced the illness; this was followed by neuralgic headache and terminated in asthma. But out of the many hundreds of cases of rheumatic fever which have passed under my review, I have no note of any instance of asthma, so that this association is only interesting on account of its rarity.

7. *Lead-poisoning.*—Asthma has been described in connection with lead-poisoning. Thus, Lewy records a case,† and Riegel‡ mentions two others, but thinks both of them doubtful, so that if this form really exists it must be very rare.

8. *Affections of the Nose.*—The proof of the causal connection between asthma and affections of the nose constitutes the chief advance which has been made of recent years in our knowledge of asthma.

Nasal symptoms in association with asthma were of course familiar to the early writers on the subject, especially in connection with that form which is now called *hay-fever*, and numerous instances are recorded by them in which the asthmatic attack was produced by scents, smells, emanations, dusts, or powders; still it is to Voltolini that belongs the credit of first clearly proving the causal relation between asthma and nasal disease. In 1872 he

* 'Clin. Medicine,' New Syd. Soc., 1867, i, 641.

† 'Oester. Ztsch. f. prakt. Heilk.,' xvi, 6, 1870.

‡ 'Ziemssen's Handbuch,' IV, ii, 1875.

published* cases in which asthma was associated with nasal polypi and was cured by their removal. In 1874 Haenisch records other cases in which, after removal of the polypi, the asthma returned with a recurrence of the local disease on several occasions, and was each time cured by operation. The next step was made by Fraenkel in 1874, who showed that asthma could be associated not only with polypi, but with any chronic nasal catarrh. A series of papers followed from this and other authors (Hartmann,† Bresgen,‡ Joal,§ &c.), conclusively establishing these facts. In 1882 Hack‖ wrote his important paper, which, from the attention as well as the opposition it excited, led to increased study of the subject and to a great advance of knowledge. He maintained that asthma and many other nervous affections, *e.g.*, migraine, epiphora, paroxysmal catarrh, &c., were reflex neuroses originating in local affections of the nose, and that they were associated with and depended upon a peculiar erectile turgescence of the cavernous tissue on the anterior portion of the inferior turbinate bones. Fraenkel,¶ however, showed in 1884 that the reflex neuroses could originate from any part of the mucous membrane of the nose, and that the swelling of the cavernous tissue was not a necessary condition; and this view has since been confirmed by many observers. Recently Schmiegelow has published a very complete monograph, containing not only the record of a large personal experience, but a comprehensive and critical review of the whole subject.

It is obvious in the first place that asthma is not always associated with nasal affections, and that when the two conditions concur, their association *may* be purely accidental. It may also be assumed that asthma depends upon a peculiar condition of the nerve-centres, through which the paroxysms may be excited by various peripheral irritations of which the nasal is only one among many co-ordinate groups; and, lastly, that even in those cases in which local nasal disease formed the starting point of the affection, the asthma may, if of long duration, acquire as it were

* For references see Fraenkel, 'Berl. klin. Woch.,' xviii, 16, 1881; and Schmiegelow, 'Asthma,' London, 1890.

† 'Deut. med. Woch.,' 1879, 373.

‡ 'Volkmann's klin. Vorträge,' 1882, No. 216.

§ 'Arch. gén. de Méd.,' 1882, i, 440.

‖ 'Wien. med. Woch.,' 1882 and 1883.

¶ 'Volkmann's klin. Vorträge,' No. 242.

an independent existence and continue after the nasal affection, its original cause, has been cured.

In those cases in which asthma stands apparently in close causal relation with nasal affections, the local affections met with have been the following:—polypus, chronic rhinitis of all kinds, including even the atrophic form, growths on the septum or turbinate bones, nervous catarrh, paroxysmal sneezing, and other irritations without organic lesion.

The statistics showing the frequency with which asthma and nasal affections are associated fall into two groups: in the one a series of asthma cases are taken and the number of instances determined in which nasal affections are found; in the other a series of cases of the different nasal affections are taken and the number of instances determined in which asthma is met with.

Group 1.—Lublinski* found that out of 500 asthmatics 143 had pathological alterations in the nose, treatment of which cured 27 and improved 13.

Group 2.—A. Nasal polypi—

Baecker† met with asthma in 9 cases out of 310 = 2·9 per cent.
Hering „ „ 7 „ 200 = 3·5 per cent.
Schmiegelow „ „ 31 „ 139 = 22 per cent.
26 of these having true spasmodic asthma and 5 asthmatic symptoms.

B. Chronic rhinitis—

Fraenkel out of 32 cases met with asthma in nearly one-third.
Schmiegelow out of 514 cases found asthma in 40 = 8 per cent., true spasmodic asthma in 34, and asthma-like symptoms in the remaining 6.

Statistics therefore go far to prove a probable connection as regards cause and effect between the two affections. The results of treatment prove the connection in some cases conclusively.

A.—In some cases complete and permanent cure of the asthma has followed complete cure of the nasal affection. In others the asthma has been cured for the time, but has returned with a recurrence of the nasal affection, and has again yielded to the appropriate local treatment.‡

* 'Deut. med. Woch.,' 1886, Nos. 23 and 24.
† *Ibid.*, 1886, Nos. 26 and 27.
‡ *Ibid.*, 1886, Nos. 28 and 30.

	No. of cases.	Cured.	Improved.	No result.
Lublinski .	143	27 (=18·8 per cent.)	13 (= 9 per cent.)	
Heymann .	54	29 (=55 per cent.)	14 (=27 per cent.)	11 (=18 per cent.)
Schmiegelow	56	32 (=57 per cent.)	11 (=20 per cent.)	7 (=12 per cent.)

The following illustrative cases are interesting as being two of the earliest recorded by Voltolini and Haenisch respectively. A man, aged 40, had suffered from severe attacks of asthma for four years. He was found to have numerous nasal polypi, which were removed, when the asthma disappeared and did not return. The second case was in a woman, aged 23, who had been the victim of asthma since 10 years of age. She was greatly relieved by the removal of several polypi from the nose. They recurred and the asthma returned several times, but each time operation relieved, and in the end with the final cure of the polypi the asthma was completely cured.

Besides asthma, Schmiegelow records cases in which other respiratory subjective symptoms were relieved by treatment of the nasal condition. A feeling of weight upon the chest in two cases, unpleasant pressure on the epigastrium in another. In a fourth case the treatment of chronic rhinitis with the galvanic cautery or chromic acid produced at first a violent tickling cough, and then gave sudden relief to a feeling of shortness of breath and oppression on the chest which had been long complained of.

B.—In another very interesting group of cases local treatment of the nose either aggravated the attack of asthma, when it was present, or excited it during the otherwise free interval.

In Schmiegelow's series this occurred in no less than seven cases, and in one instance followed the insufflation of a powder consisting of a mixture of nitrate of silver and starch.

C.—Still more remarkable is the fact that in two cases Schmiegelow actually produced asthma in persons who had never had asthma before. In one case, that of a woman aged 25, who suffered habitually from violent attacks of catarrh and sneezing with profuse nasal discharge, a very severe seizure was brought on, lasting for several hours, by simply syringing the nose with a (1 per 1,000) solution of corrosive sublimate.

In the other case, that of a man aged 28, who was under treatment for epistaxis, the most violent attacks followed the cauterisation of a part of the septum with chromic acid. The paroxysms

recurred at night for several nights until the reaction in the nose consequent on the treatment had subsided, and then they ceased. These cases are the more interesting and important, because asthma has never been produced experimentally in animals at all, and I do not know any other instances of the kind in man.

Although it is clear from what has been stated that there is more in the relation of nasal irritation to asthma than has been until recent years believed, and although most marked relief is given in some cases, still, so far as we know at present, it is impossible to recognise beforehand, with certainty, the cases in which relief will be given and those in which it will fail. Inasmuch as the cure of chronic nasal disease involves prolonged, tedious, and often somewhat distressing treatment, hesitation may well be felt before advising patients to submit to it, until ordinary methods have failed, unless the indications pointing to the nose as the source of irritation are much more evident than in most cases they really are.

D.—In another series of cases in which, without any obvious nasal disease, there have been various nasal symptoms, such as sneezing, itching of the nose, or catarrh, the asthmatic attacks have yielded easily to the insertion into the nostril of a tampon of cocaine or menthol.

This is the place in which may be conveniently considered the relation in which asthma stands to paroxysmal sneezing, paroxysmal coryza, and to hay-fever. Not only, as already stated, may these affections accompany asthma, but they may alternate with it.

Hay-asthma, which there is no reason to regard as a special form of asthma except so far as it is excited by a special cause, introduces hay-fever into the group of allied affections, for in a given case or series of cases of hay-fever there may be traced every degree from the simple summer catarrh to well-marked asthma; and, if in families with an asthmatic history it can be shown that sometimes the one and sometimes the other affection develops in the descendants, the proof of the pathological connection between them seems to be complete.

Ringer* gives one or two family histories of this kind:—

* 'Therapeutics,' chapter on Arsenic.

Grandfather. Asthmatic many years, died at 80.

Father

Son, aged 35. Hay-asthma from a baby, but greatly influenced by locality.

Daughter, aged 24. Hay-asthma since 20.

Grandson, aged 10. Asthma since 3, associated with sneezing and itching in nose, influenced by diet.

Mother. Asthmatic when young, then free for several years. Hay-asthma for last ten years.

Son, aged 19. Paroxysmal sneezing and itching of nose, brought on by dust and sunlight, but only in the summer. Is free in winter.

Son, aged 30. Asthma and bronchitis since 5, preceded by cold in head and sneezing. Coryza in the day time; chiefly asthma at night.

Ringer also quotes a case of a man, aged 35, whose grandfather had asthma, who himself suffered from paroxysms of sneezing for many years and then became asthmatic, the asthma alternating with the sneezing.

I know moreover of one family, in which the grandfather suffered from asthma from early life and died at 64. The father of the family and his son suffered from attacks of paroxysmal sneezing, which often came on without cause at any time of the year, but were almost invariably excited by sudden exposure to bright sunlight in the summer.

9. *Affections of the Pharynx.*—The pharynx being so much less sensitive than the nose, it might be anticipated that reflex neuroses from the pharynx would be far less common. Two cases of the kind are recorded by Porter.* In one, a man aged 40, asthma was associated with pharyngeal polypi and disappeared on their removal. The polypi recurred and with them the asthma, but the cure was finally complete. The second case he records was in connection with chronic tonsillitis.

10. *Affections of the Neck and Mediastinum.*—Cases of this kind in which asthmatic attacks occur are usually referred to irritation of the trunk of the pneumogastric either in the neck or within the thorax. Thus asthma might be met with in connection with goitre, new-growth, or enlarged glands in the neck; and with aneurysm, mediastinal tumour, or enlarged glands within the

* 'Med. Record,' New York, 1879, iii, 345.

thorax. It is often impossible to exclude the effects of pressure and to be sure that the attacks are really asthmatic at all.

Eustace Smith, for example, refers asthma in children in the great majority of cases to the pressure of enlarged bronchial glands, and in this way explains the frequent improvement or cure by cod-liver oil, iron, and residence at the sea-side.

In my own experience, asthma in connection with any of the affections mentioned, either in the neck or in the thorax, is extremely rare; and although it is true that tracheal obstruction from pressure may give rise to paroxysms of dyspnœa, which may wrongly be termed asthma, still in the majority of cases there is sufficient difference in the character of the attack to enable the diagnosis to be correctly made, and the case to be referred to its proper category.

11. *Affections of the Skin.*—There are two reasons for the interest excited by the question whether there be any relation between asthma and affections of the skin; the one because of the well-known effect of skin-irritation or skin-shock upon the respiratory centres, the other because there can be traced, in many of the writings on this subject, an under-current of belief that the asthmatic paroxysm is in these cases due to the development in the bronchi of similar pathological conditions to those observed in the skin.

Trousseau was, I believe, the first to draw attention clearly to this connection, the class of skin affections being in his opinion chiefly of a herpetic or eczematous nature. Although Hyde Salter did not actually recognise this association, still a considerable number of the cases he records (nearly 8 per cent.) yield a history of some skin-eruption. It is quite of recent years that most of the cases have been described, and the most complete summary of them up to the date of the paper is to be found in Dr. Bulkley's article in the 'British Medical Journal' of 1885.*

Out of 4,000 cases of skin diseases, Bulkley found asthma only in thirty-seven, *i.e.*, about 0·9 per cent., those cases being excluded in which the eruption was due to syphilis or to one of the exanthemata. Gaskoin's statistics, which yield a percentage of 7·05, are vitiated by the fact that he does not sharply distinguish between asthma and other forms of dyspnœa. The skin-affections with which asthma has been associated are in the first place

* 'Brit. Med. Journ.,' ii, 1885, p. 954.

urticaria and eczema, which yield by far the largest number of instances; next, herpes and some of the exanthemata; in the third place, but very rarely, psoriasis, syphilis, and certain drug eruptions; and, lastly, a miscellaneous group, the various members of which only require to be named—acne, purpura, erythema nodosum, alopecia areata, xeroderma, and lupus vulgaris—in most of them the association being obviously merely accidental. Considering how common skin affections are and how long asthma lasts, it is clear that a large number of asthmatics would suffer from skin-affections, without anything more than an accidental association existing between them. General statistics, therefore, will be of little value, and the only cases which would be of use in this respect are those in which the two affections stand in a manifest clinical relationship to each other, for example, where the asthma and the skin eruption appear or disappear together or alternate with one another.

Eczema.—Bulkley's statistics give a percentage of 1·33. That is, he met with asthma in 1·33 per cent. of his eczema cases. Trousseau stated that when asthma developed in adult life a history of some troublesome skin affection during childhood was often obtained, and quotes a case of a lad who had suffered much from asthma as a child, and became asthmatic at the age of 17. Dr. Charles West says that he has never known eczema in children to be extensive and long-continued without a marked liability to asthma being associated with it. In all such cases we must remember how extremely common eczema is in early life, and how great the chances are in any case of asthma that a history of eczema might be obtained if it were specially enquired into.

The following are some of the cases* often quoted:—

(1.) Eczema of the face in a boy of 6 years in whom asthma developed every time the eczema disappeared (Caillant).

(2, 3.) Two cases in which eczema started at 2 months of age and was followed by asthma, in one of the cases at the age of $2\frac{1}{2}$ years (Stephenson).

(4.) Eczema at 14 days, dyspnœa following each improvement of the eczema (Balfour).

(5.) A family in which all the members suffered from eczema between the first and second dentition, accompanied with asthma,

* For reference see Bulkley, *loc. cit.*

both affections passing off after the completion of the second dentition (Cunningham).

(6.) Eczema and asthma in a child, developing together and cured together (Thorogood).

It will be observed that all the cases quoted occurred in children. I do not know of any case of this kind in the adult; if one was met with it would be important to bear in mind the relation of eczema to gout.

Urticaria.—Of Bulkley's cases 7 per cent. (5 out of 68) had asthma, 8 others had some bronchial symptoms. I am surprised at such a percentage, for it does not accord with my experience. Out of a very large number of cases of urticaria, though complaint has frequently been made of discomfort in breathing, I have never met with a case of true asthma, and the published cases are very few. The most severe case of dyspnœa occurring with urticaria met with was in a man remarkably susceptible to urticaria from articles of food, whose breathing in a bad attack became alarmingly short; but the dyspnœa was not asthmatic in character.

Trousseau records a case in which urticaria was coincident with asthma and disappeared with it. Warner* describes another case in which urticaria developed with patches on the fauces; asthma and severe vomiting occurred several times during the illness, which in the seventh week was complicated with rheumatic fever. The asthma, fever, and urticaria all disappeared together on the use of salicylate of sodium, having resisted all other treatment.

A case of recurrent urticaria, associated with asthma, is published by Ungar,† and others by Schnitzler and Fenekovy.

In one of Schnitzler's cases,‡ a woman, 30 years of age, was attacked suddenly during her third pregnancy with sneezing; after which urticaria and asthma developed. From these she had never suffered before: the two previous pregnancies had been perfectly normal.

The most remarkable case I know is that of Fenekovy. The patient was a woman, 39 years of age, who was attacked with nasal catarrh and frequent sneezing. This lasted for one or two days, and was then followed by urticaria and an attack of asthma. When the urticaria subsided the bronchial secretion became

* 'Brit. Med. Journ.,' 1885, i, 483.

† 'Berl. klin. Woch.,' 1881, No. 48.

‡ 'Wien. med. Presse,' 1884, 1565.

copious and the asthma ceased. These symptoms always followed in the same order, and recurred at intervals of four or five months for eight years, interrupted only by pregnancies, when the intervals were shorter and the attacks more severe.

Herpes.—The association of asthma and herpes is mentioned by Trousseau, and later by Waldenberg, but I do not know of any conclusive case on record.

The Exanthemata.—Measles is often spoken of as the starting point of asthma, both in the child and in the adult; but it is rather with the bronchitis that accompanies that disease than with the rash that the asthma should be connected. Among Salter's cases is one in which asthma followed the disappearance of a small-pox eruption.

Erythema.—The only case I know is mentioned by Bulkley as occurring in the course of erythema nodosum.

Psoriasis.—Thorogood records a case of hereditary asthma in a brother and sister, which disappeared as psoriasis developed; and Theodore Williams* a case of asthma in a syphilitic patient, which resisted treatment until the appearance of a specific psoriasis suggested the use of iodide of potassium, when both the rash and the asthma were cured.

In connection with the use of iodide of potassium may be mentioned a case in which that drug was freely administered without effect until acne was produced, and then the asthma yielded.

This short review of the facts demonstrates how slight the evidence is upon which the assumed connection between asthma and affections of the skin rests. In most cases the association is obviously accidental; in others both affections were probably the joint effects of some common cause; and the instances in which a causal connection might be even conceived to exist are so few that they throw no light upon the nature of the disease.

12. *Affections of the Nervous System.*—There are, I believe, no instances in which asthma stands in anything more than an accidental relation with any recognised disease of the spinal cord or medulla. Nor are there any cases in which asthma has been proved to depend upon any actual gross intra-cranial lesion. Still, a sufficient number of cases have been recorded in which asthma has

* 'Lancet,' 1873, ii, 328.

been met with in various forms of cerebral affection to make this association more than a mere coincidence.

Salter gives an account of a case of hydrocephalus in which, during the last four days of life, the child suffered from two paroxysms of dyspnœa of an asthmatic character. Graves described a similar case in a child suffering from convulsions.

Salter again describes a case in which asthma alternated with epilepsy, and was on each occasion preceded by the usual aura, the only difference being that asthma followed instead of the fit. In a case of Trousseau's an asthmatic patient lost his asthma after an attack of gout, at the age of 21, but suffered ever after from hemicrania.

Eulenberg* records similar cases, in which asthma alternated with hemicrania, and also angina pectoris.

The most interesting contributions to this subject have been recently made by Savage† and Conolly Norman,‡ in respect of the relationship between asthma and insanity. In many instances the two affections alternated, the patient being free from asthma as long as the insanity lasted, and becoming subject to it again on returning to a right mind.

Savage's cases are the following :—

I.—A woman, aged 34, without any insane inheritance, had been a patient in an incurable hospital for many years with chronic asthma. She became for the second time insane, and lost her asthma. She continued insane until an attack of asthma came on, and then began to recover. During convalescence she had but few attacks of asthma, but when well the asthma returned with greater violence than ever.

II.—A female, aged 28, with a history of insanity on the mother's side and of phthisis on the father's, became asthmatic at the time of menstruation, and continued so for six years, when she became insane. During her insanity she was free from asthma, but with mental health the asthma returned. She had two subsequent attacks of insanity, with the same sequence of events.

III.—A female, aged 32, without insane inheritance, suffered from asthma from puberty until a few months before an attack of insanity. During this time she lost her asthma, and when she became insane she said it had gone to her brain.

IV.—A female, aged 64, without insane inheritance, had suffered for twenty years from chronic bronchitis and asthma. She suddenly became

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* 'Nervenkrankheiten,' 1871.

† 'Insanity,' 1889.

‡ 'Journ. of Mental Science,' April, 1885.

excited and free from the chronic ailment. She grew suicidal and violent, but remained free from asthma.

Conolly Norman's cases are the following :—

I.—A female, aged 45, had had asthma from 25, became insane, and lost the asthma. She remained insane for two and a half years before the asthma returned, since when she continued both asthmatic and insane, but there was no relation between the asthmatic attacks and the mental condition.

II.—A male, of 50, had been well until the age of 43. He then became suspicious and depressed. These symptoms were all removed by the occurrence of asthma, to which he was ever after liable, but without any return of mental disease.

III.—A male, of 32, had had almost daily attacks of asthma from childhood. The asthma disappeared for four days, when he became depressed and threatened to kill his mother. For three weeks he continued in the same mental state, when the asthma returned. His mind then became healthy and continued so.

IV.—A male, of 20, always weak-minded and probably asthmatic, became maniacal and lost his asthma for three years. He then became quiet and the asthma returned, but the mental condition remained feeble.

V.—A female, aged 40, had been asthmatic all her life. She became suddenly insane and the asthma left her for six months. It then returned and the insanity passed away and did not recur.

VI.—A male, aged 30, had been asthmatic for many years. The asthma became less severe for two years; then delusions set in and the patient became insane. For two and a half years, while at the worst, the asthma was absent. Then he passed into a chronic demented state, with frequent attacks of asthma.

VII.—Kelp* records a similar case in a man aged 28, with a history of insanity on both sides. After suffering from asthma for seven years, for which he took large amounts of morphine, he became insane but free from asthma for seven months. The asthma then returned, and the patient recovered his senses and continued mentally well, but subject to asthma.

One interesting case of the kind has come under my own observation. The whole family is highly neurotic. The mother developed asthma late in life, about the age of 60. One son was a drinker, and two others guilty of the greatest excess both in wine and women, and of these one died of very acute phthisis; one daughter, highly emotional, was asthmatic for more than twenty years, and about the age of 45 passed into a condition of extreme neurasthenic emaciation and prostration, but continued unusually free from asthma for many months afterwards, at the

* 'Zeitschrift f. Psychiatrie, xxiv, 4.

end of which time she had a short attack of acute mania. From this she only very slowly rallied, but all this time remained free from asthma.

Asthma stands in the same relation to hysteria that it does to insanity, *i.e.*, patients may be subject to alternate attacks of hysteria and asthma; but cases of this kind are quite as rare as those of insanity. What is commonly called *hysterical asthma* is not asthma at all, but a respiratory neurosis of an entirely different kind met with in hysterical patients. The respirations are of very great rapidity, 80 or 90 or even over 100 in the minute, panting, gasping, and noisy, and often more numerous than the pulse-beats. The breathing is deep and the chest expands to the full, so that the amount of air passing in and out is greatly increased. As the result the cheeks and lips are red and florid, and there is not the slightest cyanosis and no true dyspnœa. The paroxysms are usually of short duration, but may last two or three hours. I have seen one case in which the condition was more or less persistent for many weeks, and in many of the attacks the respirations were considerably in excess of the pulse.

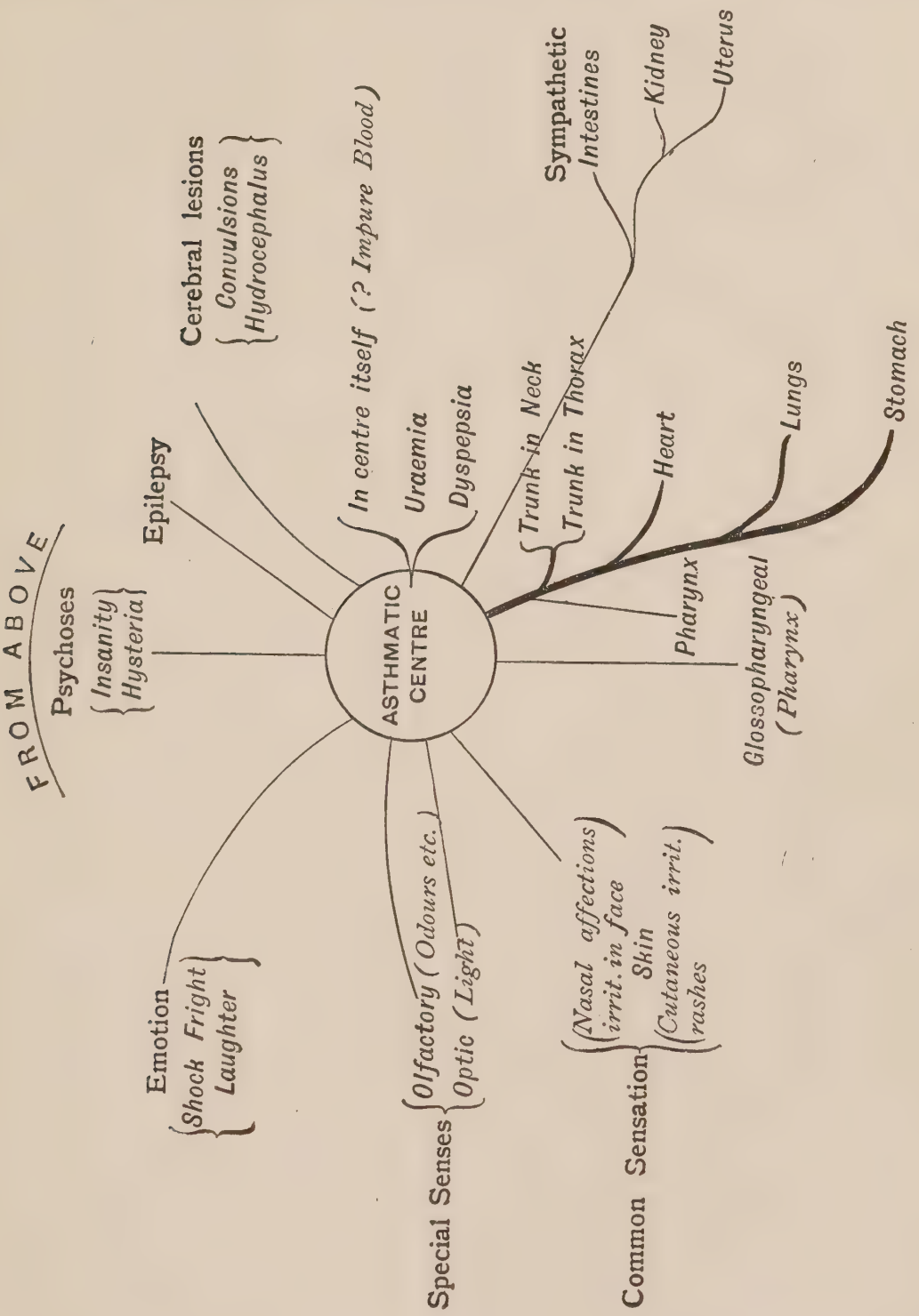
From the consideration of the affections with which asthma is associated, it is easy and natural to turn to the *exciting causes.*

There is hardly anything which has not in one case or another excited an attack of asthma, and the resemblance of asthma to epilepsy in this respect is so close that the conclusion seems almost irresistible that asthma, like epilepsy, depends upon an unstable condition of the nerve centres in which the explosion may be excited reflexly by a great variety of stimuli. Where this centre is to be sought for, whether in the medulla or brain and whether the instability depends upon an organic or simply upon a functional lesion, future investigations alone can determine.

Assuming the existence of such a centre, the paroxysm might of course originate in it spontaneously, but it would be much more likely to be excited by some irritation reaching it from above or below, a view which could be represented in the following diagrammatic form.

(*a*) *From the centre itself.*—In most cases excited by the circulation through it of blood containing some irritating impurity.

It seems almost necessary to assume this origin for those cases in which asthma develops in connection with dyspepsia, but not till



an hour or two after the taking of food. This is also the probable explanation of asthma occurring in the course of uræmia, though it must not be overlooked that alterations in the tension of the

vessels or other changes in the circulation may possibly play their part.

(*b*) *From above.*—To this category would belong (*α*) all those cases which are provoked by emotions, whether of pleasure or pain, by fright or shock, and by immoderate laughter; (*β*) those connected with various cerebral neuroses, *e.g.*, insanity, hysteria, hemicrania, &c., convulsions and epilepsy; (*γ*) those associated with gross disease, *e.g.* hydrocephalus.

Emotion may cure as well as excite the paroxysm. Thus cases are recorded in which the visit to the doctor has stopped the attack, as entering a dentist's house often stops the toothache; and another, where a patient suffering from severe asthma was alarmed by his sister falling suddenly in a faint, and at once jumped up and ran to call for assistance, though immediately before the dyspnœa was so severe that movement was impossible.

(*c*) *From below.*—(*α*) Through the *special sense* nerves.

The *optic* is as rare a source of irritation as the olfactory is common. Bright light often brings on an attack of sneezing and sometimes an attack of asthma, but the cases more commonly quoted depend rather upon an absence of light than upon an excess, some patients being unable to sleep without a light, and, if a paroxysm comes on in the dark, finding relief on lighting a candle.

In connexion with the *olfactory*, we find attacks brought on by odours of nearly every kind, the smell of violets or the rose, of coffee, camphor, or even roast hare; animal emanations from cat, dog, guinea-pig, and horse; pungent smells or fumes as of pitch or burning sulphur, fog, and mist; in some cases by dusts of almost any kind, in others only by the dust of some special substance, of which the most remarkable is ipecacuanha; the fluff of fur or feathers. And here is the natural place to mention the effect of the pollen of certain grasses to which the attacks of hay-asthma have been referred.

Many of the nasal irritations are, however, connected rather with the *fifth* nerve than with the olfactory. It is the fifth probably which is at fault where the attacks commence with nasal symptoms, *e.g.*, sneezing, itching of the nose, and coryza, and again where organic affections of the nose are present, such as polypus or rhinitis.

(*β*) Through the *pneumogastric* nerve—either by irritation of its trunk in the neck, or within the thorax, or through almost

any of its branches, except perhaps the superior laryngeal; the pharyngeal, as in the cases quoted of pharyngeal polypus; the pulmonary, as after the inhalation of dusts, fog, or mist, or after bronchial catarrh; the gastric, where the attack stands in relation to dyspepsia or to some special idiosyncrasy in regard of certain articles of food, *e.g.*, beer, sweets, &c.; and, lastly, the cardiac.

(γ) Through the *sympathetic*—from the intestines, as with worms and constipation; from the kidneys, though uræmia generally, as already stated, belongs to another category; and from the uterus or generative organs.

(δ) Through the *cutaneous* nerves—as, for example, by exposure of the face to a cold wind, by cold feet, by the application of cold or even of heat (*e.g.*, poultice) to the skin. Here may be mentioned a remarkable case I have met with in a lady in whom the slightest puff of cold air on the back of the neck was sufficient to produce the attack at any time. Lastly, in this connection the relation of asthma to certain skin affections may be noted.

A long chapter might be written on the multiplicity and variety of the exciting causes of asthma, but it is evident that, if the theory suggested be adopted, the interest shifts from the peripheral exciting causes to the position and nature of the central predisposing condition or lesion, if there be one. It would take me beyond the scope of the present communication were I to speak of the symptoms of the disease; but a careful study of them appears to me to render it impossible, after what has been stated, to arrive at any other conclusion than that the spasm of asthma is a very wide-spread one and affects many sets of muscles, the bronchial perhaps first and foremost, but still not alone, and, as well as the bronchial, also the diaphragm and the other inspiratory muscles, including, perhaps, some of the larynx. To this wide-spread spasm may be added, as an accessory, more or less vasomotor disturbance leading to hyperæmia or actual swelling of the bronchial mucous membrane, which may increase the symptoms, or even, as in the catarrhal form, be predominant.

Such an extensive spasm of so many different muscles can only be of central origin. It must be in the great majority of cases of a reflex nature, and in support of this theory all that has been said in relation to the exciting causes of the attack comes in.

The irritation which excites the spasm may originate in almost any centripetal nerve, from the pneumogastric in the first place through almost any of its branches, except, perhaps, the superior laryngeal, through the olfactory or optic, through the fifth, or common cutaneous nerves of the skin, and through the sympathetic.

To sum up then—asthma must be regarded as a reflex neurosis, the symptoms of which are spasm of the bronchial muscles, of the diaphragm, and of other inspiratory muscles associated in many cases with more or less vaso-motor disturbance in the bronchi.

The theory necessitates the assumption of a peculiar unstable condition of the respiratory centres, the nature of which is at present unknown, but which constitutes the essential predisposing cause of the disease, and which in all probability is the only heritable part of it.

In all this the resemblance of asthma to epilepsy is very close, and Hughlings Jackson* does not shrink from curtly defining asthma to be "a respiratory convulsion," and I presume that he regards it as possibly, in many cases, a congenital affection, for he proceeds to attribute it to "the discharge of imperfect (? small) respiratory centres."

It is interesting to find that this theory brings us back to the view originally propounded in his 'Zoonomia' by Erasmus Darwin, who stated that *asthma convulsivum* had the same character as all other cramps and epilepsies, and that it could originate like them from nearly all distant parts of the body.

Dr. Thorowgood agreed with the author of the paper as to the nature of the asthmatic paroxysm, except with regard to the statement that the diaphragm was depressed. Often in a severe attack of asthma he had observed the diaphragm to be drawn up; sucked up, as it were, into the thorax. He mentioned the case of a medical man lately seen in whom this elevated position of the diaphragm was very marked. The first point in treating asthma was to try and find the reflex from whence it started. The attention now given to conditions of the nasal passages had proved a decided step in this direction, and a case was referred to in which removal of nasal polypi had very much improved the condition of a man in Victoria Park Hospital, who for years had been troubled by fits of severe asthma, associated with nasal polypi. He also referred to asthma in connection with affections of the skin, and quoted a case where a chill on the surface brought on bad asthma in a few minutes. He also related the

* 'Brit. Med. Journ.,' i, 1889, 159.

cases of two children troubled with most violent paroxysms of cough, both of whom lost the cough at once on the appearance of the eruption of urticaria. In some cases where the asthmatic reflex centre appeared to be in the stomach, very much benefit had followed the administration of small doses of nitrate of silver. It seemed that an over-sensitive irritable state of the gastric mucous membrane kept up the asthma, and the nitrate of silver acted as as a local sedative in curing this condition.

Dr. DE HAVILLAND HALL agreed with Dr. West in thinking that the definition of asthma should be founded on the symptoms rather than on the pathology of the affection. The one given by Dr. West nearly coincided with Hyde Salter's. He was of opinion that the stomach might be the starting point of an asthmatic paroxysm in two ways : first, by the immediate irritation of food in the stomach ; and, secondly, from the absorption of irritant matters into the blood. He mentioned a case in which emetics gave more relief than any other kind of treatment. In speaking of the connection between diseases of the nose and asthma, he described the case of a lady with chronic hypertrophic rhinitis combined with asthma ; he would have treated the case with the galvano-cautery had the patient not been pregnant. Such marked relief followed the administration of citrate of caffeine and bromide of potassium that, after her confinement, local treatment of the nose was unnecessary in this case ; there was evidently no connection between the nasal trouble and the asthma. He was much struck by the number of cases of asthma in which relief was obtained by attention directed to concomitant disease of the nose ; but he could not call to mind a single case in which a complete cure of well-established asthma was effected by nasal treatment alone. He was in the habit of teaching students to regard asthma as a symptom rather than as a disease ; by this means they were led to investigate the cause of the complaint. In speaking of the theory of vaso-motor paralysis as a cause of the paroxysm, he pointed out that in some cases there certainly was turgescence of the nasal mucous membrane during the attack of asthma, and hence it might be inferred that the same condition obtained in respect of the mucous membrane of the bronchi.

Dr. FELIX SEMON said that in his opinion the relationship of asthma to all possible forms of nasal affections was by no means so clearly established as might be concluded from Dr. West's remarks. That some association of the kind existed—especially between nasal polypi and asthma—he would certainly not deny, but both its frequency and its importance appeared to him, from his personal experience, to be vastly overrated at present. The most unsatisfactory feature of the present situation was that we possessed no certain means of distinguishing the cases likely to derive benefit from nasal treatment from those—in his own experience unfortunately the majority—in which only temporary improvement or even no result whatsoever would be achieved. Even when the appearances were present in the nose, which by ardent disciples of rhinology were supposed to be pathognomonic of the association in question, no promise at all could be given as to a cure of the asthma being effected by intra-nasal treatment. That by this condition of things a liability to gross abuse of nasal operations in such cases was engendered would be obvious to every one. The proper course to follow under such circumstances—as a certain connection after all evidently existed, and as by discouraging, in apparently suitable cases, patients too much, one might possibly deprive them of their only chance of getting cured—appeared to him, to put the treatment to the patients in the light of a

pure experiment, telling them that good results did sometimes follow, and leaving to them to decide whether or not they would undergo the treatment. In conclusion, he briefly reported an obscure, fatal case in which asthma alternated with signs of irritation of the laryngeal and cardiac branches of the pneumogastric nerve.

Dr. Theodore Williams observed that the views contained in the paper were those of the late Dr. Hyde Salter, with some additions rendered necessary by recent advances. The neurosis theory of asthma was generally held at the present day; but to explain all asthmatic cases, the factor of diseases of the blood must not be overlooked, otherwise how could the alternations of gout and asthma, psoriasis and asthma, and syphilis and asthma be explained? The large proportion of asthma consequent on inflammatory affections of the lung must not be forgotten, such as often chronic pneumonia, bronchitis, pleurisy, and phthisis; and he assigned the asthma in these cases to swelling of the bronchial glands due to absorption of the inflammatory products during the process of resolution. The bronchial glands thus affected in the neighbourhood of the trachea pressed on the anterior and posterior pulmonary plexuses, and hence the asthmatic spasm. With regard to phthisis and asthma, many cases of chronic phthisis became asthmatic, developed emphysema largely, and it was only on post-mortem examination that the traces of old phthisical disease could be recognised.

November 17th, 1890.

ON THE DEPOSITION OF THE CRYSTALLINE URATES IN THE TISSUES CONSIDERED AS A SEPARATE PATHOLOGICAL INCIDENT, WITH A SUGGESTION FOR A DISTINCTIVE NAME.

By Sir Wm. Roberts, M.D., F.R.S.

Of all the phenomena associated with gout, by far the most definite and characteristic is the deposition of the crystalline urates in the joints and elsewhere. The occurrence of such deposits is usually regarded as pathognomonic of the gouty state. But the converse of this proposition is not accepted. We believe that a person may be truly gouty, and yet be free from uratic deposits; and clinical observers have always recognised the existence of a pathological state, to which the names of suppressed, latent, or larval gout have been applied, in which no uratic deposits can be discovered. On the other hand, the collateral conditions associated with uratic deposits are extremely diverse. These conditions sometimes differ so much from each other that they certainly would never have been classed together as varying forms of

one and the same disease, had there not been this single connecting link of uratic deposits.

Again, although the arthritic troubles of gout may be reasonably attributed to the direct effect of the mechanical irritation produced by the deposition of the crystalline urates, there are other troubles assumed to be truly gouty, which cannot, with our present knowledge, be thus explained—namely, the visceral and nervous disturbances which constitute an important part of the symptomatology of that nosological complex or totality which we call gout.

Further, uric acid exists in the body of the gouty in two very different states—namely, in a state of solution in the blood, and precipitated in the solid form in the tissues. To the former of these conditions the term lithæmia has been applied, and it is invoked to explain the occurrence of the visceral and neurotic disturbances just mentioned.

Moreover, we cannot ignore that there are pathologists among us who are unable to limit their view of gout to the effects of uric acid. They look upon the disorder as a complex constitutional state, having wide bearings in all sorts of directions. To them the obtrusion of uric acid, whether dissolved in the circulating fluids, or deposited in the solid form in the tissues, only constitutes a single manifestation of a multiform deviation from health. The most diverse views have been enunciated as to the nature of the initial error in gout. Some regard gout as primarily a disturbance of innervation, others as an altered type of nutrition. In some cases gout presents itself to us as if it were a form of plethora; in other cases as if it were a mode of senescence or growing old, analogous with atheromatous or mineral degeneration or with fibrosis. Some consider gout as, initially, a defect of the excretory powers of the kidneys for uric acid, others as primarily due to a diminished alkalescence of the blood; and one observer contends that gout is essentially a necrotic process in the tissues, induced by uric acid in solution, and that the deposition of the crystalline urates is secondary to the necrotic process.

Amid these conflicting views there is one fixed point in the controversies on gout around which all arguments revolve—namely, the deposition of the crystalline urates in the tissues. No speculation can get rid of this plain fact of morbid anatomy; nor can there be any doubt that these deposits, quite apart from their

mode of origin, are directly answerable for a well-marked group of clinical and pathological manifestations.

All these considerations seem to lead up to the idea that it might be well, in the interest of clear conceptions respecting gout, and as likely to contribute to the promotion of definite lines of further investigation, if we were to isolate in our minds the precipitation of the crystalline urates as a detached and independent pathological occurrence, separate from all antecedent and collateral incidents, and erect it into a precisely definable symptom or physical sign apart from everything else. This is a proposal quite in accordance with what has been done in other departments of medicine, and has greatly conduced to the progress of pathology and clinical research. Time was when the presence of sugar in the urine was regarded as synonymous with diabetes, and the presence of albumen as synonymous with Bright's disease. But by isolating these conditions under the names of glycosuria and albuminuria we have been enabled to attain to a greater precision in our ways of thinking and speaking about these two diseases. Glycosuria is now regarded in the light of a symptom, and we must add something thereto to complete our conception of classical diabetes; and we recognise that glycosuria may exist as an element in other morbid conditions besides diabetes. So with albuminuria. We know that this symptom may coincide with a variety of disorders. Another example in point is the separation of clotting in the veins from phlebitis, with which it was formerly confounded. It is now recognised that this incident may be due to other causes than phlebitis; and it has accordingly been isolated under the distinctive name of thrombosis.

Following these precedents, and others that might be cited, I venture to suggest that uratic precipitation should be separated from its associated connections, and dignified with a name of its own. For, trivial as the question of name may appear, it is quite certain that the distinctness of our comprehension of any notion is immensely favoured by designating it with a name. By this means vague notions are condensed into definite conceptions. I have puzzled myself not a little over this problem of nomenclature. The thing to be designated is the precipitation of the crystalline urates in the tissues or fluids of the body. The name I have finally decided to recommend to you is *uratosis*—compounded of the term *urate* with the Greek termination *osis*. I am afraid the

word is something of a barbarism; but a highly competent classical friend of mine assures me that it does not offend very far in this direction. It resembles in construction and in force of meaning such well-known words as tuberculosis, thrombosis, adiposis, and melanosis. The chief merit of a name, however, is that it is adapted to express clearly and precisely the meaning intended to be conveyed, and is handy and convenient for service. This merit may, I think, be fairly claimed for the word uratosis, and in order to illustrate its use I shall venture to employ it in the course of the further remarks I have to make. We should, I think, gain several advantages, besides that of avoiding a certain amount of circumlocution, from the adoption of this proposal.

In the first place, we should bring into clearer relief the distinction between uric acid dissolved in the blood and uric acid precipitated as crystalline urate; or, to use our new phraseology, the distinction between lithæmia and uratosis. The differences between these two conditions are, I think, important, and deserve, perhaps, more attention than they have hitherto received. In lithæmia the exact state of the uric acid is not authentically known. We are sure that it is not in the free state, because it is chemically impossible for uric acid to subsist in the alkaline blood-serum without entering into combination with the bases contained in it; but whether that combination be a quadrurate or a biurate, or a mixture of the two, or sometimes one and sometimes the other, we do not know. These two combinations are strongly contrasted in several respects: the quadrurates are highly soluble in blood-serum, and cannot be thrown down therein in the crystalline state, while the biurates are very sparingly soluble in that medium, and are readily thrown down in it in the crystalline form. On the other hand, in uratosis, we know exactly what we have to deal with: uratic deposits are composed of crystalline sodium biurate —that and nothing else. Again, it is an open question whether uric acid dissolved in the blood exercises any injurious effects at all. It appears not improbable that uric acid is as innocuous when dissolved in the blood as we know it to be when dissolved in the urine. But we do know of a certainty that when uric acid is precipitated in the tissues as crystalline urate, it does produce some injurious local effects. Another distinction between lithæmia and uratosis is this: Lithæmia is only an exaggeration of a normal condition, for traces of uric acid always exist

in healthy blood; but uratosis, even in the minutest degree, is pathological. There is yet another point of interest in this connection, though at present only of speculative interest, to which I desire to draw attention. It is quite possible that precipitation of the crystalline urates may occur not only in the form of the coarse masses with which we are familiar in and about gouty joints, but also in the form of minute stars or detached crystals, which can only be detected with the microscope; and it may turn out that the visceral neuroses, the thrombosis and embolism, and the occasional sudden death, which are encountered in gout, are not the result of any diffuse lithæmia, but are the consequence of actual uratosis, that is to say, are due to the precipitation of minute crystals of sodium biurate (microscopic uratosis), either in the substance of the organs or in the blood itself. I hazard this conjecture on the ground of the two following observations:—Sir Alfred Garrod found that the blood-serum of one of his patients contained uric acid to the extent of 1 part in 5714. On the other hand I found, by direct experiment, that when blood-serum was artificially impregnated with uric acid to the extent of 1 part in 5000 there could be detected in it, after a certain lapse of time, minute stars and needles of sodium biurate. It would thus appear undoubted that in some cases of gout the blood may, though probably very rarely, become so laden with uric acid as to be within striking distance of a general uratosis throughout its entire mass. The establishment of the possibility of such a condition would obviously enable us to explain the occurrence of some very obscure incidents in the history of gout.

Secondly, the adoption of the present proposal would enable us to discuss more freely and with greater precision of language the true relation of uratic deposits to the etiological factors which are concerned in their production. According to current views, these deposits are regarded as an infallible sign of the existence of the gouty diathesis, however diverse the associated conditions may be. Hence we are in the habit of speaking of *ordinary gout* and of *saturnine gout;* would it not be a more accurate expression of the facts to speak of *gouty uratosis* and *saturnine uratosis?* It is not easy to believe that lead-poisoning produces really the same constitutional diathesis as that which exists in true gout. It seems easier to believe that the gouty diathesis and plumbism, while differing in all other respects, have one tendency or vice in

common, namely, the tendency to uratosis. The possession of this one vice would necessarily give to two otherwise radically distinct primary disorders a very close resemblance in regard to their symptoms and physical signs, because these latter depend almost entirely on the direct local effects of the uratosis which is common to both. This line of argument might be illustrated and strengthened by drawing an analogy between uratic deposits and urinary concretions. A large group of cases of uric acid gravel and calculus are the undoubted offspring of gout; but there is also another large group of cases, occurring chiefly among the poor and among rice-eating populations, which have no kinship with gout, and are the result of a totally different set of etiological factors. Nevertheless, the clinical and pathological phenomena in the two groups, depending as they do on the mechanical irritation induced by the concretions, are practically undistinguishable. Here, then, we have a closely analogous example, in which two essentially distinct constitutional states produce identically the same morbid precipitation and the same sequential morbid effects.

On the other side, it might be contended that this line of argument is inconclusive, and is directly rebutted by established facts relating to the hereditary transmission of gout. For, as is well known, sthenic gout in the grandfather may reappear in the grandchild as atonic or poor gout. These two conditions differ from each other quite as much as does ordinary gout from saturnine gout; almost the sole point of resemblance between them is the occurrence in both of uratic deposits, and yet they are undeniably of common constitutional origin. To this objection it may be answered that gout is not, in reality, a homogeneous nosological unity, but a loose bundle of morbid tendencies. And it is quite in accordance with what we know of the laws of heredity that these several tendencies should be transmitted in unequal degrees, or that some of them should not be transmitted at all, while others were transmitted with increased intensity. In this way it might come to pass that a grandchild should inherit from his plethoric gouty grandfather his tendency to uratosis, but not his tendency to plethora, just as he often does inherit the characteristics of his grandfather's eyes and nose, but not those of his forehead and chin.

Then again, it is admitted that there are other factors which have considerable etiological force in the production of uratic

deposits, besides the gouty diathesis and lead-poisoning, namely, senescence, nervous shocks, local injury, and trophic changes. Two or more of these may act in combination in determining the occurrence of uratic precipitation. Adopting the nomenclature suggested, we could speak of a uratosis of purely gouty origin, or of a uratosis in which a gouty tendency was reinforced by lead-poisoning, by senescence, by shock, or by local injury; or we might speak of a purely saturnine uratosis, or of a uratosis in which saturnine poisoning was reinforced by a previously-existing gouty tendency, and so forth. By this mode of expressing ourselves we could conveniently indicate the concurrence of several factors in producing uratic precipitations, none of which alone were adequate to determine their occurrence.

Lastly, we should gain something in regard to a clearer apprehension of therapeutical indications. We could, on the one hand, aim at minimising the incidence of the predisposing cause, be it the gouty diathesis or lead-poisoning, or whatever else might eventually come to be regarded as the most important predisposing factor. Or, on the other hand, failing in that direction, we could aim directly at the uratosis itself. In this latter way we might succeed in preventing the more serious consequences of an irremovable predisposition, just as in uric acid gravel we are able, by alkalising the urine, to guard our patient against calculous accidents in circumstances where we cannot remove the causes predisposing thereto. Uratosis is, like gravel, of and for itself a disastrous pathological accident, and it may induce consequences in the joints or in the kidneys of a permanent character, which far exceed in gravity, and far outlast, the etiological factors which originally gave rise to it.

And should it turn out at last, after further inquiry and investigation, that there does subsist an absolute and invariable solidarity between uratic deposits and the gouty diathesis—and I wish to express no positive opinion on this point—we should lose nothing by adopting these proposals. On the contrary, we should distinctly gain in facility of description and in clearness of conception in regard to the main incidents of the gouty state.

I have said enough to bring the subject fairly before the Society, and I wish to elicit the opinion of the Fellows on two points, namely, first, whether it be desirable to erect the precipitation of the crystalline urates within the body into an independent symptom

or physical sign, with a distinctive name; and, secondly, whether the name suggested is worthy of approval.

GLYCOSURIA, DIABETES, AND INSANITY.

By George H. Savage, M.D., F.R.C.P.

Though I do not expect to find a material basis for every form of mental disorder, yet I admit that very many phases of nervous and mental disease depend upon general physical states.

We must all recognise the influence of good digestion on temper and mental balance, and we are equally familiar with the depression and instability associated with bad digestion or painful disease, and I will go one step further, and say that the general physician and surgeon are alive to the fact that most morbid bodily states have special nervous and mental aspects.

The hopefulness of the consumptive and the irritability of the gouty are well marked instances of the connection I refer to. In the following paper I do not want to add one to the numerous classes of the insane. I do not want to establish any peculiar forms of mental disorder to which the term "diabetic insanity" is to be prefixed. We shall attain no really useful end by multiplying such names, and shall, in many cases, leave the student with a dead name instead of a vital relationship.

I do wish, however, to point out the relationship which has been found to exist between some cases of diabetes and some cases of neurosis; probably we have yet not reached the underlying causes of the connection between the two, but the recognition and recording of facts may lead to a more just appreciation of two diseased states which are associated with the increase of conventionality necessitated by our present modes of life.

Next, I do not wish to claim originality for my work. I shall later quote what Dr. Maudsley wrote years ago on this subject, but I think that since he wrote other facts have been learned, and any way the recognition of the likeness between the causation of diabetes and insanity has not been wide or general.

I shall draw almost entirely on my own personal experience at Bethlem Royal Hospital, and in private practice.

First, then, I believe from my experience that there is not infrequently some connection between diabetes and insanity. I

have not time or knowledge enough of the subject of diabetes to give an exhaustive description of the various supposed causes of diabetes, and I fear that in some of my descriptions I shall be accused of putting down as diabetes what might be more justly considered as glycosuria; but my opinion is that it is quite impossible to draw any hard-and-fast line between the two conditions, there being connecting links passing between them extending from simple temporary glycosuria to typical and fatal diabetes. It must be remembered that the presence of the neurotic diathesis modifies bodily symptoms, and this will be seen in the examples to which I shall refer; for one or more of the most common symptoms of diabetes may disappear with the onset of mental trouble. I shall throughout treat diabetes as a group of symptoms rather than as the definite expression of a definite pathological cause.

I do not wish to claim all cases of diabetes as having a neurotic origin, and therefore I cannot be blamed for wishing to make it a neurosis any more than I can be supposed to look upon all rheumatic fever as of nervous origin, because in some cases it alternates with mental disorder. Diabetes may have a nervous origin, and diabetes may have a nervous aspect or mode of expression, and it is my object to make clear these points. Central nervous disease may give rise to diabetes. Insanity, too, may depend on a similar central disease, and in some cases both insanity and diabetes may appear during the course of the disease.

It has been shown that in lower animals irritation of certain parts of the brain will produce glycosuria; injuries to the head in men are, I believe, occasionally given as causes of diabetes, and in one case I have seen diabetes follow an apoplectic seizure. Prolonged worry and anxiety, and other causes of nervous exhaustion, will produce both diabetes and insanity.

Writers on general medicine, such as Bristowe, recognise that insanity occasionally supervenes on diabetes, while Fagge and Pye-Smith have traced similarities which exist between diabetes, gout, and some nervous troubles; and F. Taylor points out the hereditary character of diabetes, and that similar causes produce diabetes and insanity. I do not think it at all necessary—if I had the time—to go in for all the references to the subject; all I have wished to do is to make clear that it is recognised that some causes of diabetes have nervous alliances.

It is rather disappointing to find that very few asylum physicians

have recognised the connection which exists between diabetes and insanity.

Dr. Clouston, almost alone among our British alienists, following in the steps of Maudsley, and he only imperfectly, as I shall have later on to point out.

In 'Ziemmsen,' Senator says: "It is unmistakable that there is a connection due to hereditary predisposition between diabetes and diseases of the nervous system, particularly epilepsy and mental affections." The cases, he says, which confirm this opinion are few but certain.

Langiewicz found epilepsy allied to diabetes in a very significant way, and Lockhart Clarke reported a case of epilepsy associated with diabetes, though I fancy this case was really one of general paralysis of the insane.

I think it well to quote the first thing which attracted my attention to the relationship existing between diabetes and insanity from Maudsley's 'Pathology of Mind:' "Diabetes is a disease which often shows itself in families in which insanity prevails: whether the one disease predisposes in any way to the other or not, or whether they are independent outcomes of a common neurosis, they are certainly found to run side by side, or alternately with one another, more often than can be accounted for by accidental coincidence or sequence. For the present, I am content to note the fact that the children of a diabetic parent sometimes manifest neurotic peculiarities, without devising any explanation, which must be hypothetical. This we know: that diabetes is sometimes caused in man by mental anxiety, that it is produced artificially in animals by irritation of the fourth ventricle and some adjacent parts of the brain; and that a great number of diabetic patients die of phthisis. Perhaps I might set it down as a true generalisation that the morbid neurosis, when it is active and gets distinct morbid expression, may manifest itself in four ways:—(1.) Disorder of sensation, for example, paroxysmal neuralgia. (2.) In disorder of motion, for example, epilepsy. (3.) In disorder of thought, feeling, and will—mental derangement. (4.) In disorder of nutrition, whereof diabetes is the earlier and phthisis is the later stage."

I feel that I have very little beyond facts to add to this generalisation, but I think it is worth while bringing before this Society the results of my more recent observations.

Dr. Dickinson and Dr. Hale White both independently examined the urine of a large number of patients of unsound mind; and, though the former found many with traces of sugar, I must say that my experience leads me to believe that there is no general relationship between glycosuria and insanity, but that in very various mental states we may have both sugar and insanity; thus in my Manual I refer to three insane patients who had sugar in their urine.

One was suffering from brain hypochondriasis, the second from post-apoplectic dementia, while the third was suffering from puerperal insanity.

The facts which form the basis of this paper were partly derived from the cases book of Bethlem Royal Hospital for the past five years, for the use of which I have to thank my friend and successor, Dr. Percy Smith; and, besides these facts, I have others derived from private practice. All point in the same direction, and before giving particulars of cases I must refer to the subjoined tables.

Diabetes is more common among the more wealthy classes, and insanity associated with diabetes occurs in the families of the well-to-do; but I was rather astonished to find that among the patients admitted into Bethlem Hospital from 3 to 5 per cent. had diabetic relations. In consulting practice, I should think the percentage considerably higher.

In drawing up the tables of patients who had diabetes, I found the direct inheritance from parent to child not so common as the indirect relationship: thus, of 40 insane patients who had diabetic relations, 8 of them only were the parents, 8 uncles, 7 aunts, 14 brothers and sisters, the others being grandparents and cousins.

I think, then, that a large number of insane patients will be found to have diabetic relations. I cannot give any idea as to the number of diabetic patients who have insane relations, and I shall be glad to hear the experience of others on this point.

40 insane patients had diabetic relations as follows:—

11, parents and grandparents.
14, brothers and sisters.
12, uncles and aunts.
3, cousins.

10 patients were both insane and diabetic.

12 patients had both neurotic and diabetic heredity.

1 insane patient had both parents diabetic.

Next, as to the more clinical parts of the question, Dr. Clouston has pointed out that with diabetes there may be insanity of a more or less definite type running its course *with* the diabetes. I should prefer to say that in a few cases diabetes has a mental aspect. In some cases of diabetes, generally those past middle life, the weakness of diabetes has added to it despondency and misery, the patients dreading ruin, and thinking they are forsaken by their friends. These patients may be suicidal, and, instead of having an increased appetite, they may have a loathing and distaste for all food. The two disorders may make slow and steady progress, or the sugar may become lessened in quantity, or even disappear. I believe it is not very uncommon for diabetic patients who are sinking from exhaustion to lose the sugar before death, with or without the occurrence of coma.

A form of melancholia, then, may come on in the course of diabetes, without affecting that disorder. It must not be forgotten that in a few cases of puerperal insanity glycosuria may be present for a time, I presume as a result of the cessation of lactation, but I have not met with any case in which this appeared to be the starting point of diabetes.

That sugar will occur in puerperal cases has also been observed by Dr. W. M. Ord.

The presence of sugar in cases of insanity, as I have said, is rare. I have the record of one middle-aged man who had diabetes, and at the same time most marked brain "hypochondriasis," *i.e.*, constant hypochondriacal complaints of "brain feeling," and, what is also interesting, another patient of mine, a victim to brain hypochondriasis, is the child of a diabetic father.

Beside the occurrence of insanity in diabetic families, it is very noteworthy that one or all of the symptoms of diabetes may disappear on the occurrence of mental disorders. In this, of course, diabetes is not special, for acute diseases may run their course without evident symptoms when the mind is disordered, so that phthisis or peritonitis may develop without cough or pain.

Rheumatism, asthma, and gout may alternate with mental disorder, each giving place, as it were, to the others, and so it may be with diabetes and insanity. The diabetes may be replaced by simple melancholia, by mania, by delusional insanity, and even by general paralysis of the insane.

It is important to notice that, just as in the insane connections of diabetic patients the form of mental disorders is mostly of the depressed type, so with the alternation the patients generally have melancholia or delusional insanity.

I will next give a few examples of cases in which the alternation occurred.

A young woman, with no special known cause, began to grow thin, notwithstanding her very large appetite. She was sent to a general hospital, where she was found to be suffering from diabetes. For this she was dieted, and treated I regret I cannot say by what drug. For a time she improved in general health, but almost suddenly she became dull and melancholic; she was restless and sleepless; fancied something was going to happen to her. At the same time she refused food.

Hearing that she was suffering from diabetes, I was anxious to obtain some of her urine, but it was long before I could obtain any, as she was so suspicious that she would only pass water in the closet. When the urine was obtained, it was found to be of normal specific gravity, and free from sugar. She was fed with ordinary food, and after some months began to be more cheerful. For a time no sugar reappeared, but after her mental convalescence the diabetic symptoms reappeared, and continued for some time, once more to be replaced by melancholia, which ran its course as before, leaving her sane, but diabetic. I lost direct news of the case, but indirectly I heard that she died of phthisis.

Case of Diabetes alternating with Melancholia.

W. W——, admitted into Bethlem. Married. 54. No insane or diabetic relations. A commercial traveller, said to be sober. Had a previous attack of insanity nineteen years before, of which he recovered completely. He had been mentally depressed and sleepless for two weeks before his reception. His urine had been 1040, and full of sugar. He believed he was ruined, that is, he felt that he was a ruined man, though he could argue that he was not, and for a moment would appear convinced that the idea of ruin was a delusion.

He sank and died. No post-mortem was permitted.

J. C——, male. Married. 73. Gardener. Admitted into Bethlem, 1879. He had formerly been intemperate. No relations known to have been insane or diabetic. He had suffered from bronchitis, and had for several years been diabetic. Three months before admission he became sleepless, and developed delusions. He thought he was being poisoned, and called in the police. He steadily lost strength, the bronchitis became worse, and he died four months after admission.

The brain was much wasted, and there was considerable quantity of subarachnoid fluid present, and the spaces round the vessels were distinctly large, especially in the basal ganglia.

Patient who had been Insane and had Diabetes and had Neuralgia at other Times.

Mrs. S—— was suffering from melancholia, with delusion about her husband.

She recovered her reason, and was discharged. While insane the amount of urine became normal, and there was little or no sugar. Yet on recovery the urine again became voluminous, and loaded with sugar. When at home she wrote to me complaining of violent neuralgic pains under the left eye and the top of the head, beside other vague pains in limbs and extremities. She was rendered nervous and unstable by these neuralgic troubles.

I lost further sight of her.

The next case is, from many points of view, the most interesting, for diabetes of long standing was replaced by organic brain disease, again to reappear before the fatal termination of the brain disease.

General Paralysis of the Insane and Diabetic.

H. J—— in 1885 was noticed to have diabetes, and also symptoms of general paralysis of the insane.

He had unequal pupils ; loss of expression ; tremors of tongue and lips ; difficulty of articulation ; some difficulty in walking ; handwriting very shaky. He had very strong sexual desire, and had not been temperate.

He had two epileptiform seizures, which chiefly affected his left side, and which left him with some weakness of that side. He was treated by Dr. Pavy, and was placed on strict diet. Two years later it was found that the diabetic symptoms had passed off ; he could eat what he liked without increasing the amount of urine or producing any sugar.

He remained in a state of partial dementia for nearly two years before he sank and died. The sugar reappeared before the end. On admission, April, 1885, urine, specific gravity 1023. No sugar ; slight trace of albumen. June, no sugar. September, no sugar. March, bedridden ; loss of vesical control ; sugar. June, sugar present. September, coma and death.

So far, then, for some specimens of my experience. I hope, in the Transactions of the Society, to give in detail my facts and my fancies, and will now only sum up.

True diabetes may arise from local brain disease, brain injury, or brain decay. Similar conditions may give rise to insanity. Similar bodily conditions may give rise to diabetes, insanity, as well as gout.

In some cases certain of the symptoms of diabetes may be related to brain and other nervous changes, without diabetes as a whole

appearing; thus there may be nervous polyuria, or nervous glycosuria, which may be ephemeral or lasting.

In insanity itself, the presence of diabetes is not common. In the same family in which insanity occurs there are often met with instances of diabetes. The same holds good with epilepsy and the other neuroses. Diabetes and insanity, generally melancholic, may occur together.

The symptoms of diabetes are modified in several ways by the existence of insanity: thus the excessive appetite may be wanting, the quantity of urine passed may be small, and the sugar may be reduced or altogether absent.

There may be alternation between the diabetic symptoms and the nervous ones.

There is a tendency for the symptoms to assume a melancholic aspect, though there is no form of insanity which may not occur alternately with diabetic symptoms.

Dr. HADDEN inquired if Dr. Savage's observations on the occurrence of insanity in diabetes applied to all persons whose urine contained sugar. It was very important to know if insanity were liable to occur in simple glycosuria, as well as in diabetes proper.

Dr. SIDNEY PHILLIPS said abundant glycosuria occurred in a patient under his observation during attacks of epileptic mania.

Sir WILLIAM ROBERTS remarked that a sharp distinction should be drawn between diabetes and mere glycosuria. Sugar appeared in the urine in several kinds of cerebral lesions, but the symptoms did not really correspond to those of true diabetes.

Dr. SAVAGE, in reply to Dr. Beevor, said that he had no knowledge of sugar occurring in the urine after epileptic fits. It might occur from time to time in such cases, just as albumen might, but he did not think it at all common. As to the suggestion that a general nervous storm, which ends in insanity, may be associated with irritation of a glycogenic centre, he hardly felt prepared to accept such an interpretation. The term irritation was wanting in definition, and could hardly be of service. In reply to Dr. Hadden, he would again refer to the early part of his paper, in which he gave a general interpretation of the term "diabetes" that he had used. He looked upon it as a convenient group of symptoms and little more. Some of the group might be absent, and in the cases under discussion that was one of the most important points, that the symptoms might be masked. In answer to Sir W. Roberts, he would repeat that there were doubtless some of the cases to which he had referred which hardly deserved the name diabetes, but should be called cases of glycosuria. Dr. Herringham had asked what was meant by alternation? He repeated that there were certain persons who had diabetes, and who, on losing one or all of the symptoms of this disease, became insane, and who again, on becoming insane, became diabetic.

December 1*st*, 1890.

ON THE STENOSIS OF THE TRACHEA AND LARYNX WHICH OCCASIONALLY FOLLOWS THE USE OF A TRACHEOTOMY TUBE; WITH REMARKS ON THE PRESENT POSITION OF INTUBATION.

By Bernard Pitts, M.C. Cantab., F.R.C.S. Eng., and William F. Brook, F.R.C.S. Eng.

The main object of this paper is to illustrate some of the causes which may prevent the return to the natural channel for breathing —after the use of a tracheotomy tube for a prolonged period—and to draw attention to the treatment of such cases. Most frequently the difficulty arises from a want of confidence on the part of the patient, and by a little encouragement and by the use of a tube with an upward opening it is easy to overcome this very common trouble. In the four cases we narrate the obstacle to natural breathing was some form of definite mechanical occlusion of the upper part of the trachea or of the lower laryngeal aperture. Such real obstruction is, fortunately, not of common occurrence, and the difficulty of overcoming it is very great, and in some cases insuperable. Some lessons may, we think, be learnt from a study of these cases. At any rate, we feel that we have gained experience, and are in a position to deal with any future cases with more precision and with quicker success. Tracheotomy in young children is usually performed high, and the cricoid is probably often divided. Unless the tube happens to fit the particular case exactly, a good deal of irritation of the back of the trachea, or even of the lower laryngeal aperture, is apt to be occasioned by the angle of the tube. The tube rarely fits comfortably if the cricoid has been divided, and it seems important to avoid this division if possible. No doubt much friction by this part of the tube is occasioned by movements of the larynx in swallowing and by restlessness on the part of the patient, and this friction would be greatly diminished if a tube with a movable collar, such as a Durham's tube possesses, were more universally adopted. Unless

there be allowance for variation, a tube which fits well in a child of a certain age will be most unsuitable for a child of the same age, but with a more deeply placed trachea. At Great Ormond-street Hospital an ordinary bivalve has been in most frequent use, and possibly this may have occasioned some of the irritation in two of the cases requiring after-treatment for stenosis. The pressure sore that is too often found on the front wall of the trachea, and which is caused by the end of the tube, has not had in any of our cases anything to do with the after-trouble; it is, however, evidence of the use of a tube of unsuitable shape.

Case 1.—Edward F——, aged 3, was admitted to Great Ormond-street Hospital on December 3rd, 1886, under the care of Dr. Cheadle, with difficulty of breathing and marked stridor. He had been an in-patient for a short time early in the year for a similar attack, from which he recovered without operation. On examination, the throat was found congested, but no membrane was to be seen. There was no evidence of any syphilitic trouble in the family. The symptoms gradually became more urgent, and on December 14th tracheotomy was performed by the house surgeon with instant relief. The tube was removed on December 17th for five hours, but had to be replaced. The child now had a sharp attack of scarlet fever with high temperature and some lung complications; and was obliged to wear the tube continuously through the attack. —February 15th, 1887: Trace of albumen; child pale and wasted. The tube was taken out, and on February 18th the wound had nearly closed and the breathing was satisfactory.—March 1st: Right side dull; breath sounds weak; much stridor. Aspirator used, and a few drops of pus drawn off.—8th: The difficulty in breathing became suddenly urgent, and the house physician had to perform fresh tracheotomy, and only just managed to pull the child round.—April 20th: Better; running about the ward; gaining flesh, but still obliged to wear the tube.—June 20th: The tube was left out on trial; spasm of glottis one hour after, and the tube had to be replaced.—24th: Mr. Lewis, the house surgeon, under chloroform, passed a Macewen's tracheal catheter with a little difficulty up from the wound through the mouth, and then pushed the lower end downwards into the trachea below the wound; the catheter was retained, but during the night the child held the tube so tightly with his teeth as to prevent the entry of air, and it had to be removed. During the next three months Mr. Lewis repeatedly passed these catheters, and when the wound became smaller he introduced the tube from the mouth down the trachea beyond the site of the tracheotomy wound. The boy could not retain the tube long, rarely more than one hour at a time, and it occasioned great distress. There is no doubt, however, that he improved; the tracheotomy opening closed, and for a short interval he was able to breathe comfortably. Mr. Lewis makes a note that he always had great difficulty in introducing the tube through the mouth.—October 17th: Had a bad attack of dyspnœa in the night, fifteen days after the last dilatation.—19th: Tracheotomy was again necessary. The house surgeon cut through a quantity of cicatricial tissue, and had some difficulty in getting the tube to pass into the trachea, owing to the narrowing of the lumen of the trachea.—November 3rd: The child left the hospital for the Highgate Convalescent Home, still wearing a silver tube, and spent the

next year either at the Convalescent Home or at his own home, wearing the tube continuously.

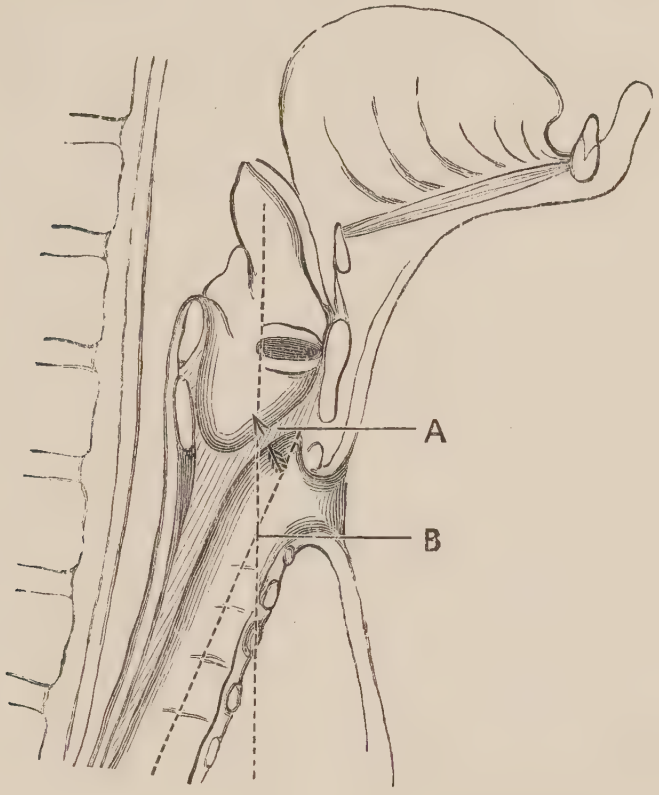
He was readmitted to Great Ormond-street Hospital on March 1st, 1889. He was then in fair condition, and able to phonate fairly well by putting his finger over the opening of the tube.—March 4th : The tube was left out for four hours ; the opening rapidly contracted, making the reintroduction of the bivalve tube difficult.—5th : The tube was taken out as usual, and a sponge tied over the opening. After forty minutes, whilst the child was playing on the ward velocipede, he became distressed and the sponge was removed, but without any relief. He suddenly lay on the floor and became cyanosed, even to his finger tips. After a short struggle he became quite still, and the staff nurse inserted the tracheal dilator, and by smacking the child succeeded in making him breathe. The tube was inserted by the house physician as soon as he arrived on the spot, and the little fellow was soon comfortable again.—August 21st : By request of Dr. Cheadle Mr. Pitts took charge of the case, and under chloroform intubated the larynx with an O'Dwyer's tube. There was no difficulty in passing the tube, and the end could be felt through the tracheotomy opening, but the breathing was not satisfactory, and it was evident that the tube did not lie straight in the trachea, but the lower end was tilted forwards and to one side. Several sizes of intubation tube were now tried, but without success ; and, as the child was rather exhausted, the tracheotomy tube was replaced, and he was put to bed. The next day, August 28th, chloroform was administered by Mr. Priestley, the surgical registrar, and the following is his description of the operation :—The wound was extended upwards, and the thyroid partially divided. Retracting the parts with blunt hooks, it was found that there was a rather thick pinky-white band of fibroid tissue, which started from just below the cords and extended obliquely downwards from right to left, and thus a sort of diaphragm was formed, nearly blocking up the lower aperture of the larynx. There was also a general overgrowth of fibroid tissue round the commencement of the trachea, and at this point the trachea was seen to bend backwards. It was found necessary to paint the interior of the larynx with cocaine, as the irritation of the manipulations caused vomiting, but this was entirely kept under by the cocaine. With a pair of curved scissors the band was now dissected away and as much of the surrounding fibroid thickening as possible. The cords were examined from below, but no growth or thickening was found higher in the larynx. A long curved tracheotomy tube was introduced and the thyroid brought together by a silver suture. Two sutures were placed below the tube, so as to raise the height of the tube, and thus try to keep dilated the part where the dissection had been carried out.—September 7th : Tube removed.—14th : Wound healed, voice improved, and breathing easy and without stridor. —October 19th : For the last fourteen days there has been increasing inspiratory stridor and occasional distress during the night. Tracheotomy was performed and patient allowed to resume his old tube life till December 16th, when it was determined to again try the effect of intubation with O'Dwyer's tube. Under an anæsthetic the largest size intubation tube was easily passed by Mr. Brook, house surgeon, and a tracheal plug was applied to the opening in the neck. This tube was well tolerated and allowed to remain in till December 18th. During this time the boy was quite comfortable, playing about the ward, and taking minced food and pudding. He remained without further intubation till January 2nd, 1890, the tracheal wound being then quite healed. The intubation tube was passed without an anæsthetic by Mr. Brook, and left in twenty

minutes. Intubation without an anæsthetic was again done at the end of the month, once during February, and for the last time on March 4th, 1890 ; the tube passed with great ease and was left in ten minutes. He was now allowed to go home, and has never been intubated since. During the last nine months he has remained quite well, and has been constantly helping his father as a fish hawker ; he uses his voice constantly in crying his father's goods. He is here to-night, and if we can get him to shout, he will show that—notwithstanding his various tracheotomies, thyrotomy, and intubations—he has a remarkably loud voice, and one well suited for his profession as a hawker.

We have narrated the case of this boy rather fully, for it was the first of the series. For more than three years he baffled all the numerous efforts made to do away with the tracheotomy tube, and it was only after the thyrotomy that it was possible to determine the exact nature of the obstruction. Dilatation of his larynx by Macewen's catheters was most carefully carried out by Mr. Lewis, and all ordinary means were tried to get rid of the tracheotomy tube and failed, except for brief periods. It was never quite clear what was the primary cause of the dyspnœa. Shortly after the original tracheotomy he had a bad attack of scarlet fever, and probably this explains the irritation set up by the tube. In the twentieth volume of the Clinical Society's Transactions several very interesting cases are recorded by Mr. Bilton Pollard where he succeeded by the use of Macewen's tracheal catheters in establishing natural respiration. In the case of E. F——, success at first seemed to attend this method, but relapse always speedily followed. By reference to the diagram (Fig. 1) it is quite easy to see why this failure occurred, and also why dilatation by O'Dwyer's method at first was useless. The cicatricial band formed almost a complete diaphragm, and the result of any form of intubation was merely to dilate the lumen of the diaphragm or else to push the diaphragm before the tube. The lesson that was learnt is that with failure by simple intubation to restore the natural passage, a free exploration of the tracheal wound should be made, and all cicatricial tissue at once be thoroughly removed, and the passage thus prepared for intubation, which should be then again employed and repeated from time to time, until all tendency to recontraction has passed away. After the thyrotomy and excision of the cicatrix in E. F——'s case, too long an interval was allowed before intubation was again used ; with more confidence in intubation the last tracheotomy would have been unnecessary. The advantages of O'Dwyer's method

over Macewen's tracheal catheters are sufficiently obvious. An O'Dwyer's tube has an increased circumference at the part wanted

Fig. 1.



A, Cicatricial tissue. The arrow shows the lumen of the diaphragm. B, The dotted lines show the alteration in the axis of the trachea.

for such dilatation. It is well tolerated by the patient for a lengthy period, and food can be taken without difficulty.

Case 2.—Charles C——, aged 9, was admitted on August 24th, 1889, under Dr. Cheadle, with membrane on soft palate and difficult breathing. High tracheotomy was performed by the house surgeon, with relief of symptoms.—August 26th : Progressing favourably till last night, when the child suffered from frequent cough and bleeding from the tube. An anæsthetic was given, and several bleeding points in the depth of the wound were secured. During the next three months all attempts to remove the tube were attended by immediate distress, even when an anæsthetic was administered.—December 10th : Transferred by Dr. Cheadle to the care of Mr. Pitts. The tracheal wound was enlarged, and a quantity of granulation tissue was found at the front and sides of the trachea at the lower border of the wound ; this was removed with scissors and a sharp spoon. The largest sized intubation tube was introduced and a rubber plug put into the tracheal wound. The tube was coughed out in half an hour and the tracheotomy tube was replaced.—11th : The house surgeon, Mr. Brook, intubated without an anæsthetic.—12th : String was removed, as it occasioned some cough and discomfort. This tube remained *in situ* from December 11th till the 18th ; it was then removed

and not replaced.—January 1st, 1890: Tracheal wound healed; voice natural.

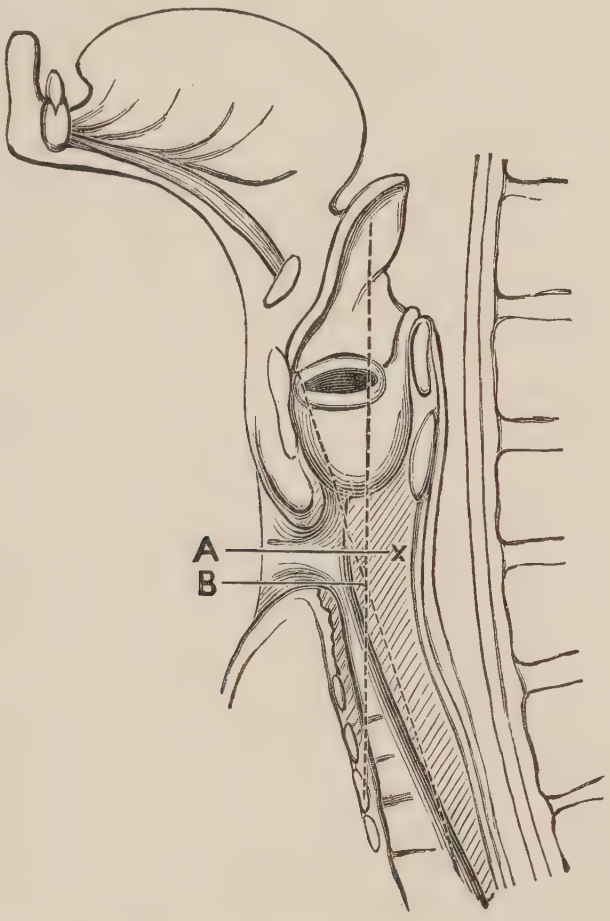
On January 10th (twenty-four days since last intubation) he had a rise of temperature, and some pneumonia at the left base. No further intubation was done, and he was discharged well on March 6th, and the voice and breathing were quite normal. He has been kept under observation since, but no further intubation has been necessary. At the present time his voice is clear, and there is no stridor even after exertion. In this case, since dyspnœa had for four months constantly attended every effort to dispense with the tracheotomy tube, it was thought right to make an exploration of the wound before employing intubation. Possibly the removal of the granulation tissue might have been sufficient, but we thought it advisable to keep up some dilatation until the surface, laid bare by the removal of the granulation tissue, had completely healed.

Case 3.—Albert H——, aged 3 years and 3 months, was admitted on July 31st, 1889, under the care of Dr. Cheadle, suffering from diphtheritic membrane on tonsils and marked dyspnœa. Tracheotomy was at once done by the house surgeon, and a large quantity of membrane was removed from the trachea.—August 4th: The wound was covered with membrane, and during the next week great sloughing of the wound went on.—9th: Much of the fluid taken by the mouth escaping by wound, sore rapidly increasing, and at the bottom of the wound the trachea to be seen lying bare for more than one inch of its extent, and through the large opening in it the posterior wall of the trachea can be seen.—10th: Rectal feeding necessary.—11th: Enemata returned, so nasal feeding substituted. Child very collapsed and almost pulseless.—September 9th: Child has slowly improved; gets up wearing tube.—October 28th: Unable to phonate or to do without tube; bougie passed through larynx.—December 12th: Dr. Cheadle transferred the patient to the care of Mr. Pitts. Intubation under chloroform. Tube retained three days, and then removed on account of rise of temperature. Tracheotomy tube replaced.—23rd: Both tonsils and some adenoids removed by Mr. Brook to facilitate intubation.—January 2nd, 1890: Intubation again under chloroform, but the tube was removed next day, as there was a rise of temperature. Small tube difficult to pass.—11th: Intubation under chloroform. Tube retained till 16th, when a larger one was passed without an anæsthetic; this was retained until January 20th, and then the largest tube but one was introduced.—28th: The largest tube was passed, *i.e.*, one suitable for a child of 12.—February 3rd: The tube was taken out, and ten minutes after the child was unable to breathe, and the tracheotomy tube was put in, the opening having been kept patent by tracheal plug.

Intubation after this was several times tried, but became more difficult, and on February 16th the child was placed under chloroform and examined, and the tracheotomy wound was freely enlarged; an examination showed that at the level of the tracheal opening there was a dense collar of cicatricial tissue encroaching on the lumen of the trachea. The trachea at the same point appeared to bend backwards, the part below forming an angle with the part above the obstruction collar, as shown in Fig. 2. The cicatricial tissue was dissected away very freely, together with the lining of the tracheotomy opening. A large tracheotomy tube was then introduced. Intubation was recommenced on February 27th; the largest size but one was left in. It was coughed out next day, but although the tracheal wound was plugged the patient could breathe comfortably through the mouth. The tracheal wound was allowed now to heal; intubation

was repeated every few days until May 7th, when the child developed measles. At this period phonation (an extremely deep note) was just possible with great effort. During the attack of measles it was found necessary to keep the intubation tube almost constantly in. Intubation

FIG. 2.



A. The cicatricial tissue in this case extended a long way down the back and sides of the trachea.

was frequently done during June, and on July 14th the child was sent home, as it was thought he might be intubated as an out-patient. He returned in three days after his discharge, a tube was put in, and he wore this at home for ten days, taking ordinary food as usual. The tube was now left out sixteen days, and then again worn for ten days ; it was then taken out and replaced and retained for another week. During the last two months the mother neglected to bring the child to the hospital. By special request the mother brought the child to see us on November 23rd. There was then some stridor and dyspnœa on exertion. Phonation only by great effort.

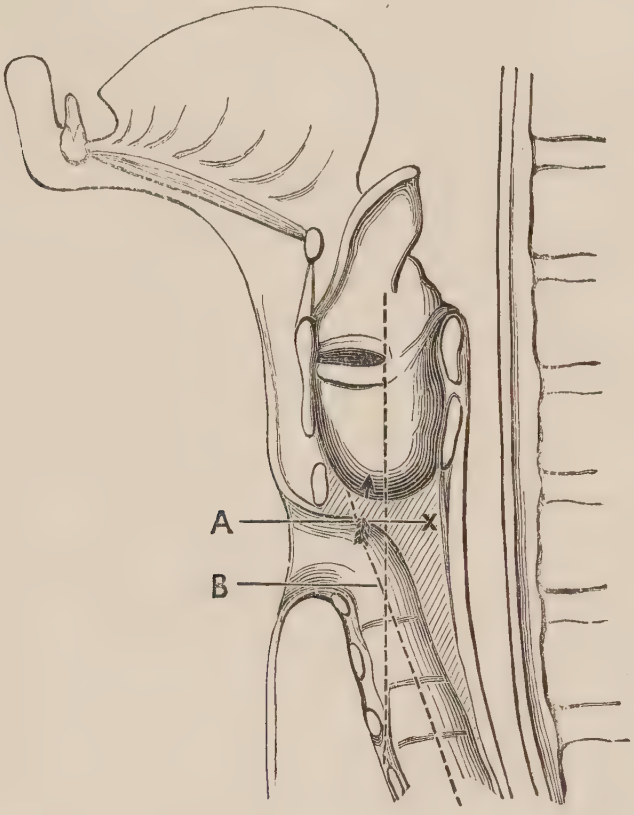
Remarks.—This case shows the great difficulty that may be experienced in the restoration of natural breathing. Considering the extensive sloughing of the tracheal wound and the necrosis of portions of the tracheal rings, it cannot be wondered at that an extensive formation of cicatricial tissue arose. Some kinking of

the trachea opposite the tracheal opening is, we believe, of not infrequent occurrence, and this, combined with the cicatricial stricture, made the case remarkably intractable. Much time was wasted in the treatment by not attending to the lesson that ought to have been learnt from the first case—namely, where intubation is not at once satisfactory, to explore for and remove, as far as possible, all mechanical obstruction. We were unwilling, after the extensive sloughing that had occurred, to meddle with the wound if we could avoid it. This case must be considered as still under treatment. There must be as much necessity for constant dilatation as in a bad cicatricial stricture of the urethra. We believe, however, if the mother had not neglected to bring the child for the last two months it would not have been in a more satisfactory condition than he is at present. The parents unfortunately considered the child so well as not to require further treatment.

Case 4.—Arthur P——, aged 9, was admitted on July 12th, 1885, to the West London Hospital, with dyspnœa necessitating tracheotomy. He remained in the hospital till February 18th, 1886, when he was discharged, wearing a silver tracheotomy tube, all attempts to dispense with the tube having failed. In June, 1886, he was admitted to the Great Ormond-street Hospital, under the care of Mr. Marsh. For six weeks frequent attempts were made to remove the tube, but it always had to be replaced, and he was discharged and told to come again in a year's time. He was readmitted to the Great Ormond-street Hospital, under the care of Mr. Morgan, in November, 1889, and we are indebted to that gentleman for permission to include the case in this series.—December 1st: Mr. Brook tried to intubate, but failed to pass even the smallest sized tube.—10th: Mr. Brook examined under an anæsthetic, and, after enlarging the wound, found that the trachea just above the level of the tracheotomy tube was obstructed by a collar of cicatricial tissue, forming a web, with an aperture towards the right side of the mid-line. After dilating the aperture with probes and catheters, an intubation tube was passed by the mouth; the end appeared opposite the wound, but projected forwards and against the lower border of the tracheal wound. The tube was therefore removed, and the tracheotomy one replaced.—14th: Under chloroform, Mr. Morgan divided the collar vertically with scissors in two or three places, after which a large intubation tube was passed.—January 11th, 1890: It has been found impossible for the boy to retain the intubation tube for any prolonged period, the point of the tube getting pushed against the anterior wall of the trachea. The wound was therefore enlarged, and Mr. Morgan dissected away a considerable portion of the collar of fibrous tissue. Intubation was carried out at intervals by Mr. Brook during the rest of January and February, but not much progress was made.—March 1st: Mr. Morgan again enlarged the tracheal wound, and more completely removed the remnants of cicatricial tissue. After this the wound was allowed to close, and intubation went on more satisfactorily. On May 8th the boy was sent home, on account of an epidemic of measles. He returned in five days, and was intubated for six hours. He came up once a week for intubation during the next

two months, and afterwards at intervals of three weeks.—November 23rd : Still some stridor on exertion. Sleeps well. No loud breathing.

Fig. 3.



Can phonate with an effort. The largest tube can be easily passed, but there is still some slight obstruction.

Remarks.—The tracheotomy tube in this case had been worn over five years continuously. The diaphragm-like web was of much the same character as in the first case narrated, except that it did not extend so high into the larynx, and that the lumen was much smaller. Like the first case, too, the necessity for complete removal of all cicatricial tissue is shown before satisfactory progress can be made with intubation. The case will probably require further intubation, and, owing to his age, with a larger sized tube than has yet been employed. It is interesting to note that, after a period of five years' inability to talk, he now, at the age of nine, articulates like a child of three.

General Remarks on the Cases.—It may be again stated, without much fear of contradiction, that when ordinary means have been used to leave out the tracheotomy tube intubation by O'Dwyer's method should be at once given a trial, and that if difficulty arises, or no definite improvement speedily ensues, then a thorough explo-

ration of the parts should be made, cicatricial tissue should be freely and completely removed, and then intubation again be tried without allowing time for recontraction. The advantages of O'Dwyer's method over any other form of dilatation are so obvious, and the risks in such chronic cases are so small, that if this form of intubation were only applicable to such cases, we would still owe a debt of gratitude to O'Dwyer for placing this means of treatment at our disposal. It may be in place here to draw attention to a few special points in the intubation for chronic stenosis. 1. As regards the force required in introducing the tube: In dealing with acute inflammation, any attempt to use force would be likely to end in disaster, but in these stricture cases a certain amount of force is absolutely necessary, and if used in the right direction can be safely employed. It is often necessary to press continuously the point of the tube against the contraction, and so there may be a danger of causing asphyxia. It is a practical point of some moment to get over this difficulty, and this can be done by withdrawing the introducer for a short distance, keeping the tube in place with the forefinger, and so allowing some slight passage of air through the tube; pressure is then again applied, and the stricture as a rule yields. 2. A much larger tube can be tolerated, and is desirable, than is laid down for the treatment of acute cases. 3. The time that the tube may be left in is well shown in the cases. The boy H—— frequently wore his tube for a fortnight, and on two occasions did so at home without the slightest discomfort or any alarm to his parents. 4. Use of the string: In all cases where the tube was expected to remain in for more than twelve hours the string was removed. This plan was adopted at the request of the older patients, who said they could eat better without it, also that its presence made them cough. 5. Feeding: As in acute cases, immediately after the first commencement of intubation pulp food was found to be the best, fluids causing cough. After a short experience all these children were able to take ordinary diet without any trouble. 6. Extraction of the tube with the extractor is always the most difficult part of the operation, and it certainly is so in these cases, owing to the unusual length of the tubes employed. This special difficulty is caused by the length of the tube *plus* the length of the beak of the extractor being greater than the distance between the cords and the hard palate, against which the angle of the extractor will thus impinge

before the point of the tube is out of the larynx. This difficulty was overcome by having a shorter beaked extractor made, and by pushing the point of the extractor as far as possible into the tube before attempting to remove the tube.

Remarks on the Instruments Used.—The following slight modifications in the intubation instruments have been made by Messrs. Weiss, at the suggestion of Mr. Brook:—The introducer (Fig. 4) used is the same as altered by Mr. Staveley, the only difference being that the tube is held on by a string passing over a button at the angle instead of a ring. The introduction of the tube was found to be easier with this than with O'Dwyer's introducer, and its advantages over the latter complicated instrument, as regards cleanliness and cheapness, are obvious. The extractor (Fig. 5) shown is much shorter in the beak than O'Dwyer's, for reasons already stated, but was quite long enough for use in diphtheritic or any other cases under 12 years of age. The special form of gag was found very useful, it was easy to introduce, and inasmuch as the flat handles lay in close contact with the face over the left malar bone and temple, the hand of the assistant which held the head on that side, having the gag beneath it, effectually retained the latter in position. The plug used for keeping the tracheotomy wound open is shown in the diagram; it is made of vulcanite with a rubber collar.

We conclude this paper with a few remarks on the present position of intubation.

a. It is obvious that intubation is unsuitable for the following class of cases:—(1) Obstruction caused by presence of foreign body; (2) active ulceration (syphilitic or tubercular) of larynx; (3) new growths, especially malignant ones; (4) obstruction due to alteration in shape or position of trachea caused by goitre or other tumour of neck; (5) obstruction by post-pharyngeal or other abscess of neck.

b. Cases where intubation seems to be directly indicated:—(1) Simple cicatricial stenosis, such as in the cases described or resulting from old syphilitic disease; (2) scald of larynx; (3) œdema of larynx; (4) acute laryngitis; and (5) for sudden spasm of glottis, such as may sometimes arise during the course of operations under anæsthetics, provided the instruments be at hand, in fact, in all cases where the trouble is likely to be of only short duration, and where there is no evidence of associated

chronic ulceration of the larynx. It may also be employed as a means of diagnosis when it is uncertain whether the dyspnœa arises from obstruction in the upper air passage, or to some condition out of reach, *e.g.*, pressure from mediastinal tumour, bronchitis, &c.

We now come to the question of intubation or tracheotomy in diphtheria, and we trust on this point to gain some information from those who have had experience on this question. We may start by saying that we have come to no absolute conclusions, and it seems to us that in this country, at any rate, there has not as yet been a sufficient trial of intubation to enable anyone to speak with great confidence on this question. We could quote a large

Fig. 4.

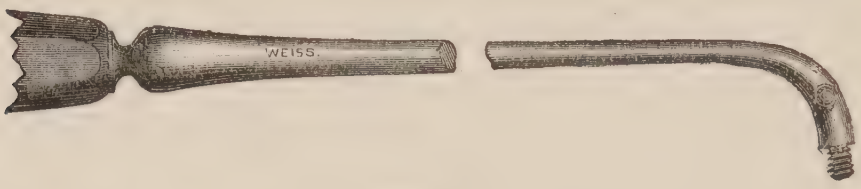
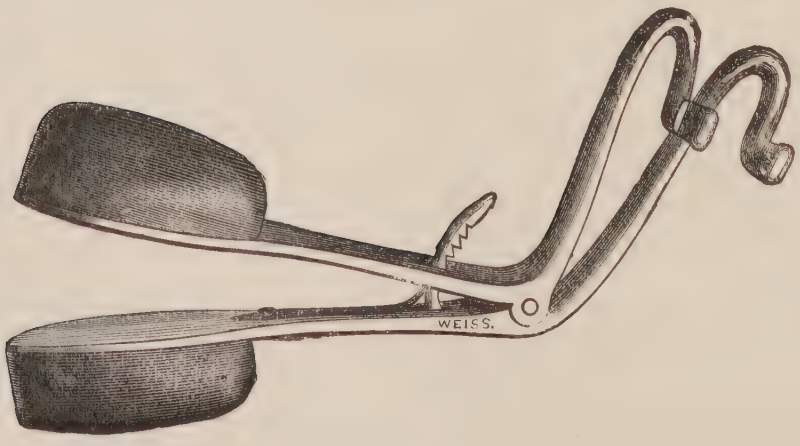


Fig. 5.

Fig. 6.



amount of American statistics, but we think the question must be settled by the success which attends the treatment of the disease as possibly modified by climate and conditions of life in this country. No short experience of intubation can be of much value in determining this question. We all know how in some epidemics a very varying degree of success has attended tracheotomy. At times there is a uniformly raised percentage of recoveries all round, at others most of the cases die; and even at the same period one hospital may be showing a comparatively large percentage of recoveries, whilst others are losing almost all their cases, and this may depend on the severity of the disease and the condition of the patients brought to the particular hospital, and also upon whether tracheotomy is done early or only *in extremis*. All this applies equally to the treatment by intubation, and, as intubation has as yet been so little practised in this country, probably the operators have much experience to gain in the necessary manipulations, and in the after-management of the cases.

We give the statistics of the results of intubation in diphtheria from three hospitals, and we hope that others will be forthcoming. Victoria Hospital for Children : 16 intubated, 7 cured (no second tracheotomies); 9 deaths (3 second tracheotomies, all fatal); percentage of recovery, 43. We have given these results first, for we believe that Mr. Staveley, formerly house surgeon there, was the first to give intubation in diphtheria a systematic trial in London, and he has already published a most excellent account of his experience. At St. Thomas's Hospital, up to the present time, 30 cases have been intubated for diphtheria ; of these 8 had subsequent tracheotomy, 4 recovering. Of the 22 cases where intubation alone was employed 8 recovered. This is a percentage of 40 on the whole number intubated. At Great Ormond-street Hospital 11 cases were intubated by the house physician, with 1 recovery. It is only fair to add that, owing to the unfortunate early mortality, intubation was reserved for those cases which were least likely to recover, and tracheotomy practised for the most favourable ones. We may mention here that Cheatham, of Louisville, has recorded 32 cases with 10 recoveries, and of the first 15 of these only 1 recovered. All will allow that the results obtained at the Victoria Hospital and St. Thomas's Hospital are far more favourable than those usual after tracheotomy, since 25 per cent. recoveries after tracheotomy may be considered good, and we may safely argue

that in the face of these results we are bound to give treatment by intubation a most serious consideration and an exhaustive trial.

The following may be mentioned briefly as some of the advantages gained by intubation :—1. The friends are not frightened by the idea of an operation, and their consent is not so necessary as for tracheotomy. 2. Intubation does not require any anæsthetic, and this we consider a most important point; for often the necessary giving of an anæsthetic in cases of tracheotomy is a direct cause of death, and always of much anxiety. 3. The operation may be undertaken at a much earlier period than is usually considered justifiable for tracheotomy, and can be far more easily performed without skilled assistance. 4. The avoidance of the hæmorrhage and other dangers attendant on tracheotomy, and the avoidance of a fresh wound, which may become infected. 5. The air before entering the lungs is warmed and filtered by passing through the natural passages, instead of entering straight through the opened trachea. 6. The question of the advantage of being able to do without a skilled nurse is a debatable one. If all goes well, her presence is not so necessary as in tracheotomy. If the tube gets blocked it is always coughed out, and generally urgent symptoms do not return for a period of an hour or longer. But, on the other hand, if difficulty of breathing arise at once, the skilled nurse is helpless, and the child must die unless the medical man is promptly on the spot. This is a danger which must be well considered before doing intubation in private practice.

We will now briefly refer to a few of the arguments against intubation in diphtheria :—1. It does not give that complete rest to the larynx that is obtained by tracheotomy. 2. If there is much membrane in the trachea it cannot be cleared out by forceps in the way that is most desirable after opening the trachea. It is, however, astonishing what large quantities of membrane may be coughed up by the child through an intubation tube or after its expulsion. Though the lumen of the tube is small, it is straight, and does not get foul, as a tracheotomy tube does, since it is kept constantly warm and moist. 3. The presence of a tube fixed tightly in an inflamed passage may, of course, in such a disease as diphtheria, aggravate the local mischief or produce ulceration at any of its points of contact; and physicians who examine the fatal cases in the post-mortem room may well draw unfavourable conclusions. Fortunately such ulceration is not of very frequent

occurrence, but certain cases of ulceration produced by the tube have come under our notice, more particularly amongst the fatal cases that have occurred at St. Thomas's Hospital. In a case at one of the London fever hospitals this ulceration went on to necrosis of the cartilages, and the tube, having lost its support, dropped into the trachea. We have also heard of one case in a provincial town where, although recovery took place (after subsequent tracheotomy), permanent occlusion of the larynx resulted, no doubt from ulceration caused by the use of the tube. Among the 10 fatal cases which occurred at Great Ormond-street, there was only the slightest abrasion of the mucous membrane in 3 or 4 cases, and definite ulceration in none. We understand also that this has been the experience in the post-mortem observations at the Victoria Hospital, Chelsea. A recent preparation of the larynx and lung is here shown, taken from a child, aged $3\frac{1}{2}$ years, who was admitted into St. Thomas's Hospital under Dr. Ord, 3 months ago, with diphtheria. Intubation was performed, and the tube worn for 4 days, and the child apparently recovered, but continued to expectorate small quantities of offensive matter, and died at the end of the 3 months from symptoms of gangrene of the lung. The larynx is healthy, but the trachea shows a healed ulcer, about the size of a threepenny piece, with evidence of destruction of cartilage beneath. The lung shows several large patches of well-marked gangrene, and it is fair to assume that this was occasioned by the ulcer, which corresponds to the position of the end of the intubation tube. 4. The frequently mentioned objection, and one which is especially in the thoughts of a beginner at intubation, is that of the possibility of detaching membrane and pushing it before the tube, and so making an obstructive flap. This may occur, but we believe from published accounts it is very common, and no one should do intubation without being prepared to proceed to tracheotomy if required. Possibly, as experience in the operation of intubation is at present very limited, by a better selection of cases and of tubes of a proper size, and by the improvements which are constantly being made in the shape of the instruments by the inventor, many of these objections may in time be lessened.

It seems to us, with our present limited knowledge, that (1) tracheotomy is advisable in those cases where there is evidence of great malignancy, or of a tendency to great extension of mem-

brane; (2) that intubation should be employed in cases which present themselves with dyspnœa as the most marked symptom of the disease, that it should be performed at an early period, and that should there be necessity for retaining the tube for a lengthened period, or evidence of any direct irritation set up by it, then tracheotomy should be substituted, and we may here remark that tracheotomy is rendered safer and easier of performance when an intubation tube is *in situ*.

DESCRIPTION OF IMPROVED INTUBATION INSTRUMENTS.

Mr. Brook then showed the intubation instruments in use at the Hospital for Sick Children, which were made by Messrs. Davis, explaining where they differed from those used by O'Dwyer. The introducer, consisting of a simple shaft, into which the pilot screwed at right angles, was the same as that first used by Mr. Staveley, and described by him in the 'Lancet,' vol. ii, 1889, p. 1292; with this difference, that the tube was held on by the string passing over a button at the angle instead of through a ring. The introduction of the tube was easier with this than with O'Dwyer's introducer, and the advantage over the latter complicated instrument as regards cleanliness was obvious. The two extractors shown were both shorter in the beak than O'Dwyer's, for reasons mentioned in the above paper, but the larger of the two was quite long enough for use in any cases under 12. For the form of gag which was used at Great Ormond-street, Mr. Brook claimed the following advantages:—That it was easy to introduce, and, inasmuch as the handles lay in close contact with the face against the malar bone and temple, the hand which held the head on that side also effectually fixed and retained the gag in position. The tubes shown differed very little from the latest shaped used by O'Dwyer. Mr. Brook mentioned that tubes were, however, still made with the shoulder projecting forward as well as laterally and backwards, and suggested that the ulcers at the base of the epiglottis, shown in a diagram of a larynx, after four days' intubation, were, probably, caused by such a projection.

Mr. Sheild said that the Society was much indebted to Mr. Pitts for having clearly demonstrated the true nature of these troublesome cases

of difficulty of breathing after tracheotomy. The operations performed by him were founded on several pathological principles, for in cicatricial contraction of any mucous canal division or removal of the inflammatory material was essential. He referred to a similar case at St. George's Hospital, when the child, however, ultimately recovered, and dispensed with the tube without operation. He suggested the possibility of muscular spasm complicating these cases. A consideration of them was important, especially in private practice, for there the little patient had not the advantage of a skilled house surgeon or nurse constantly with him. Speaking of intubation, he did not think that it would ever take the place of "emergency" laryngotomy, since the latter operation had usually to be done most unexpectedly, even with a common pocket knife. In conclusion, he believed that intubation combined with thyrotomy might prove of use in cases of syphilitic stenosis in adults, who constantly had to wear a tracheotomy tube.

Mr. Bruce-Clarke, after congratulating Mr. Pitts on the excellent results which he had obtained after opening the larynx in the cases which he had described, stated that his own experience had practically led him to the same conclusion. He had had a case under his own care in which tracheotomy had been performed two years previously in consequence of a scald. All effort to do without the tube had completely failed, nor had intubation effected any but a temporary advantage. Tracheotomy had been twice performed without any permanent good resulting. He had laid the larynx freely open, dividing the thyroid cartilage from top to bottom, and found the interior of the cavity covered with granulation tissue, with some cicatricial tissue underlying it in parts. He scraped it as clear as he could with a Volkmann's spoon, removing as far as he could the cicatricial tissue, and swabbed it out with chloride of zinc, 30 grains to the ounce. A tracheotomy tube was again inserted, and retained for forty-eight hours. After the lapse of about ten days intubation was begun, and continued at intervals for nearly four months. Since last July the child had been in the country, and no tube had been passed, but she remained perfectly well and strong, and had a very fair voice, quite clear, though not loud. As to the question of intubation, his experience was practically limited to its use after scalds, and in these cases it certainly yielded most brilliant results. For after forty-eight hours the tube could be removed, and no further trouble was experienced.

Dr. Donkin read a short abstract of six laryngeal cases intubated by Dr. Hastings, the Resident Medical Officer of the East London Hospital for Children. Three of them were acute membranous laryngitis, one laryngitis of a doubtful character (no post-mortem being obtained), and the other two were laryngeal stricture, the results respectively of a severe scald and tracheotomy some years before, and of membranous laryngitis and tracheotomy. In the first acute case of membranous laryngitis the introduction of the tube produced complete laryngeal obstruction, the effect, Dr. Hastings supposed, of the membrane being pushed down; so that the tube had to be withdrawn, and tracheotomy at once performed; the withdrawal of the tube was not followed by coughing up of the membrane, as O'Dwyer describes, and the child remained deeply cyanosed till the trachea was opened. In the other three acute cases, the introduction of the tube was followed at once by relief as complete as is given by tracheotomy, and in none of them were there any after-symptoms of obstruction in larynx or trachea, although the children had much rattling

in the throat, and appeared unable to cough up the mucus satisfactorily. For this the tube was removed and re-introduced, generally with benefit, but the re-introduction was sometimes difficult from rapid swelling taking place about the entrance to the larynx. Once or twice the tube was coughed out ; the next size larger was then introduced and found to remain, causing no more discomfort than the smaller one. The ultimate result was not successful in any of these cases, two dying from syncope, and two from the membrane spreading downwards to the pulmonary air passages. Dr. Hastings advises that, to prevent the tendency to syncope, it would be well to even tie the patients down in bed ; and recommends that tracheotomy instruments should always be kept in readiness, before intubation, in case of any blocking of the trachea. The other two cases referred to were old tracheotomies in which it had not been found possible to remove the tracheotomy tubes. Both had continued up to now to do well, without either kind of tube, after repeated intubation for a while ; one breathing now without stridor, the other with slight stridor. There is no dyspnœa in either cases, and their voices are fairly loud, although hoarse. Only one of these six children had any difficulty in swallowing ; drinking always made the child cough, and feeding through a nasal tube had to be resorted to. Dr. Hastings considers that intubation has the following advantages over tracheotomy :—1. Its greater easiness in performance and freedom from danger. 2. The absence of wound, and avoidance of shock. 3. The more ready permission of it by parents. 4. The less painful after-treatment. On the other hand, he points out that there is more difficulty in expelling the membrane than in tracheotomy, and the local treatment is carried out less efficiently. On the whole, he thinks that intubation should be resorted to first, whenever possible, but that in a certain number of cases tracheotomy will be required afterwards, and may be necessary even sooner than otherwise, in consequence of intubation. Dr. Donkin agreed with these views, and said that intubation in acute cases was not in his opinion likely to be successful when marked expiratory stridor was present, indicating probably great membranous obstruction. He did not think that the ultimately unsuccessful results of intubation for membranous laryngitis were any argument against it as a remedial measure. He felt sure, too, from considerable post-mortem experience, that tracheotomy had little or nothing to do with the lung condition found in most fatal cases. There was practically no difference between the lungs of those who die without, and after, tracheotomy. The so-called pneumonia after tracheotomy was probably always the result of the inflammatory process spreading downwards from the upper air-passages. Intubation and tracheotomy on acute membranous cases should be comparatively criticised, not from the curative, but from the temporarily remedial, point of view.

Dr. Arthur Voelcker agreed with Mr. Pitts that intubation was most successful in the cases of chronic laryngeal obstruction described in the paper, but was afraid he could not take the same hopeful view of intubation for cases of diphtheria that Mr. Pitts took. His experience was limited to the results of intubation at the Hospital for Sick Children, Great Ormond-street. During the present year there had been 50 cases of diphtheria admitted : of these 11 cases were intubated and 11 had tracheotomy performed ; of the 11 tracheotomies, 3 recovered and 8 died. Of the 11 intubations, 1 recovered, after prolonged bronchitis and suppuration of glands in the neck, and 10 died. The average age of the patients intubated was $3\frac{6}{12}$ years, that of those tracheotomised $3\frac{8}{12}$ years.

The average duration of life after the operation was almost the same after intubation as after tracheotomy ($1\frac{3}{4}$ days). The successful cases of tracheotomy were $5\frac{7}{12}$, $3\frac{10}{12}$, $4\frac{3}{12}$ years old, and the intubation case 4 years old. He had been struck in making the post-mortem examinations by the extent to which membrane penetrated along the bronchioles in cases which had been intubated, and, although a similar condition was found after tracheotomy, he was under the impression that this was more marked after intubation. He agreed with Mr. Pitts that it was too early yet to decide the merits of intubation in diphtheria on so few cases, but thought that up to the present the results of intubation at the Children's Hospital had not been encouraging.

Mr. Staveley said that he had tried intubation as a substitute for tracheotomy for the treatment of dyspnœa in diphtheria at the request of Mr. Clutton. He was at first much disinclined to adopt the procedure, but the result, seven recoveries out of sixteen cases, all being young children, had won him over, and now he naturally thought that the prospects of intubation in diphtheria were hopeful, though he fully realised that his hopes were based on a very small number of cases. He had met with no cases of ulceration of the larynx in his own patients. With regard to intubation during the administration of chloroform, he had met with one case in point. It was a case of lymphadenoma of neck, causing intense dyspnœa. Tracheotomy was being performed, and, owing to a mass of growth being situated in front of the trachea, the operation was one of extreme difficulty. Respiration, which had become more and more embarrassed, ceased before the trachea was reached. An O'Dwyer's tube was inserted without delay; respiration followed, the cyanosis disappeared, and the operation was completed with the greatest facility, and with no return of the dyspnœa. He had also performed more than one tracheotomy with an O'Dwyer's tube *in situ*, in cases where, owing to the constant cough, much loose membrane was suspected. He found the tube in the trachea a great advantage. The chloroform was taken well, the venous engorgement diminished, and the tube could be grasped through the soft parts, and the trachea thus steadied. On reaching the trachea, however, it was advisable to withdraw the tube, and open the trachea by transfixion in the usual way, rather than cut down on to the tube. There were two reasons for this :—1. The presence of the tube was liable to mask the tracheal rings. He himself had once split the thyroid cartilage, being led up by a large thyroid isthmus. 2. In cutting down on to the tube it was possible that the mucous membrane might escape division to the full extent of the incision, and serious difficulty be met with in endeavouring to insert the tracheotomy tube.

Dr. Turney confined his remarks to the use of intubation in diphtheria. He had found it associated with some dangers. He had, in one case, as he believed, thrust membrane down, so as to occlude the trachea. In other cases, after the tube had been coughed up, dyspnœa in its most urgent form had appeared immediately. In five cases he had found, *post mortem*, more or less extensive ulceration, which in some instances had been caused within twenty-four hours. In one case, the tube having been in for ten days, necrosis of the laryngeal cartilages and rings of the trachea had occurred, the tube then dropping down the trachea, and causing death. In conclusion, he advised the use of intubation in diphtheria as a temporary measure only.

Mr. W. Wyndham Powell said that he had recently tried intubation in three cases of diphtheria at the Westminster Hospital, and in the first

case the treatment was perfectly successful in combating the laryngeal obstruction. The case was that of a child aged 14 months, with a very short and fat neck, and the tube was inserted at the last moment to save life. The relief, which subsequently was complete, was not by any means immediate. The tube was taken out on the third day for rattling in the trachea. No membrane was expelled, but a little was removed from the lumen of the tube. In two hours it became again necessary to introduce the tube. On the following day it was coughed up and this time was left out for six hours, when its replacement in the larynx was necessary for urgent dyspnœa. The tube was removed and its use discontinued on the sixth day, and from that time until the child's death from broncho-pneumonia, on the ninth day, the breathing, as far as the upper air passages were concerned, was quite free and noiseless. The second case was that of a child 2 years of age, and, again, intubation was not done before the patient was deeply cyanosed. The breathing became even worse than before, and the tube was immediately removed, when a large cylindrical piece of membrane was expelled. The dyspnœa continuing, the tube was replaced, but again without relief, and tracheotomy had to be rapidly done. The patient died of syncope ten hours afterwards while sitting up in bed. In the third case, also, tracheotomy had to be substituted for intubation, which was done at the last moment without the immediate relief the condition demanded. In Mr. Powell's opinion intubation even in diphtheria would prove valuable in many cases, but, when employed, should not be delayed until the patient is *in extremis*, as is often done in tracheotomy, it being far inferior to the latter operation in giving the immediate relief necessary in deep cyanosis. The treatment, he thinks, is more especially indicated in cases where permission for operation cannot be obtained, in young babies and in patients where the obstruction is considered to be of a fairly temporary character.

Dr. STURGES desired to say one word on the use of intubation in the course of diphtheria. Its service at a later stage, namely, where there was difficulty in dispensing with the tracheotomy tube, had been sufficiently alluded to that evening. He had made trial of intubation chiefly in the case of infants suffering diphtheria, with whom, as is well known, tracheotomy is so largely fatal. He was, however, compelled to the belief, already expressed by Dr. Voelcker, that the final issue of intubation in these patients was disappointing, and occasionally it even seemed harmful in preventing the rejection of membrane. The best that could be said of it (restricting the observation to the early and active stage of diphtheria) was that sometimes it seemed to relieve the distressing dyspnœa of the patient without having any effect either in arresting the disease or saving the life. The chief use of intubation, one would suppose —though he spoke theoretically—would be in the case of laryngeal spasm, not only in infants, where it is so often fatal, but also with adults, as, for example, in thoracic aneurysm. But, unfortunately, such attacks were so sudden and unexpected that the aid of intubation would often come too late. He regretted having to say of his own limited experience of intubation in diphtheria that it was sadly disappointing.

Mr. PITTS, in reply, said that the object of the paper was to elicit further experience; he would not detain the Society by any remarks, but would like to draw attention to the fact that the cases narrated where membrane had been pushed down by the tube were first or very early cases, and were therefore possibly not quite trustworthy.

December 8th, 1890.

ON CONGENITAL CONTRACTION OF THE FINGERS AND ITS ASSOCIATION WITH "HAMMER TOE;" ITS PATHOLOGY AND TREATMENT.

By William Adams, F.R.C.S.

General Remarks.

Congenital contraction of the fingers—in most cases limited to the little finger—has not, hitherto, attracted much attention, and is, sometimes, not even mentioned in surgical text-books. The reason for this is that the cases, although of common occurrence, are generally unimportant, and, in the majority of instances, this contraction does not give rise to trouble, although in exceptional cases, at a late period, it does become an important affection. No one thinks anything of a crooked little finger, and, so commonly does it exist, that a gentleman once remarked to me, "All our noses and little fingers are crooked." And this is, undoubtedly, the popular notion, in which is embodied a certain amount of truth.

Clinical History.

This affection had better be considered in reference to three distinct stages in which this contraction is met with.

First Stage.—Although this is, undoubtedly, a congenital and hereditary affection, no deviation is observed in the little finger at the period of birth, such as we see in congenital club-foot, for example, and other congenital muscular contractions. I do not think I have seen any case in a child under 1 year of age; but I have seen many cases in children between 1 and 2 years of age. The mothers, however, have assured me they have observed it a few months after birth. This form of finger contraction occurs chiefly in girls. The first indication observed is a dropping of the second and third phalanges of the little finger, with some inclination to turn laterally inwards towards the median line of the hand; the third phalanx is generally rather more twisted than the second.

There is no evidence of muscular contraction; no contracted bands of fascia can be felt; no appearance of shortening of the skin on the palmar aspect of the fingers (see Fig. 1). The second and

Fig. 1.

Congenital contraction of little finger in first stage, from a child 3 years of age.

third phalanges can be easily restored by gentle manipulation to a straight position with the first phalanx; but they drop again as soon as the extending power is removed. This is a remarkably close resemblance to what takes place in the first stage of hammer toe. The general characters described are well represented in Fig. 1, taken from a child aged 3 years.

Second Stage.—This is essentially one of confirmed contraction, which takes place gradually if the malposition in the first stage has not been corrected by the child wearing a retentive metal splint. In this stage there is no evidence of muscular contraction, but some contracted bands of fascia can be detected in most cases. The second and third phalanges generally remain on the same line, and become more or less rigidly flexed upon the first phalanx, which is usually drawn backwards or hyper-extended, as shown in Figs. 2 and 3. The finger cannot be straightened by any moderate amount of force, and the resistance would seem to be

Fig. 2.

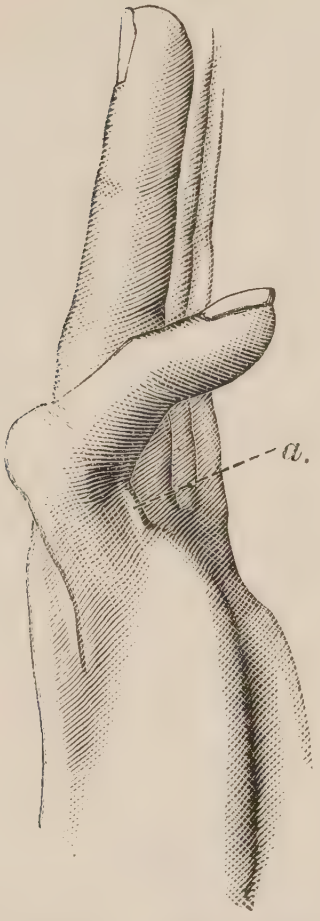
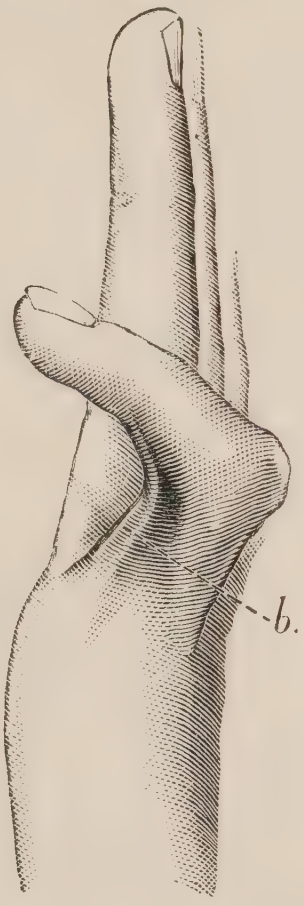


Fig. 3.



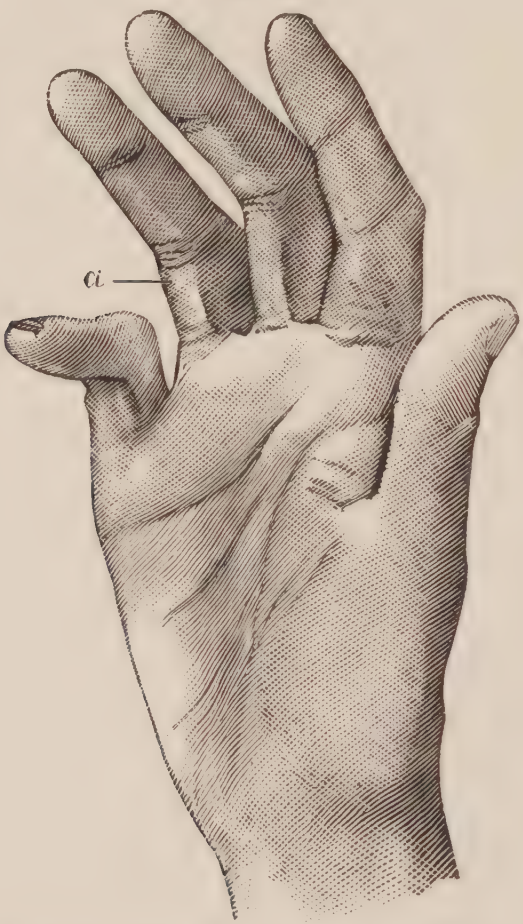
Congenital contraction of little finger in second stage. Patient aged 29 years.

partly in a contracted condition of the skin and fascia, and partly in the contracted condition by adapted shortening, during growth, of the articular ligaments. The transverse creases in the skin on the palmar aspect of the fingers corresponding to the phalangeal articulations become less distinctly marked; and the skin, somewhat atrophied and thin in appearance, seems to be short and contracted. This probably led my former colleague, the late Mr. Tamplin, to believe in congenital contraction of the skin being the cause of this malposition; but no doubt it is only one of the secondary effects of the contraction. This stage appears to be seldom reached under 7 or 10 years of age, and even then often attracts but little attention, only being regarded as a crooked family finger. Later on, however, it is found to interfere more especially with music, and further advice is sought, generally about the age of 16 years.

Third Stage.—When the congenital form of finger contraction

has not been submitted to treatment in the first or second stage, spontaneous arrest may, and undoubtedly does, take place in many cases, as shown in Figs. 2 and 3, the deformity remaining throughout life without causing much inconvenience; but this cannot be relied upon, and, in a certain proportion of cases, increase of the contraction occurs at a late period—say, from 15 to 18 or 20 years of age—and these cases present several remarkable peculiarities, so that I have described this period of increase as a third stage of the congenital contraction. In this stage the increase of the contraction is seldom confined to the little finger, but all the fingers become involved; as shown in Fig. 4; and there is evidence of

FIG. 4.



Congenital contraction of the fingers in the third stage. From a young lady aged 17 years.

fascial contraction of a peculiar kind, differing widely from the fascial contraction met with in the better-known form of Dupuytren's finger contraction, as well as a further contraction and shortening of the skin. When this increase of the contraction

commences, the second and third phalanges of all the fingers begin to bend downwards, so that the hand gradually becomes less and less useful for any purpose, and in young ladies the piano has to be abandoned. It is soon found that the fingers cannot be straightened by any moderate force, and when the attempt is made the skin on the palmar aspect seems to be short and contracted. There is also some appearance of a central flat band of fascia about three-eighths of an inch in width, running lengthwise along the first and second phalanges. The margins of this band become tolerably well defined in the central portion of each phalanx, and expanding near to the articulations are gradually lost. This is well represented in Fig. 4. As this appearance of a central longitudinal band does not correspond to any natural distribution of the digital prolongations of the palmar fascia, which we know pass obliquely forwards and backwards towards the periosteum along the inner and outer margins of the sheath of the flexor tendons, its explanation seems to be difficult. It appears to me to be due to the terminal fibres of the fascia inserted into the skin becoming involved, and contributing to an appearance of skin contraction. I formed this opinion after the first operation I performed on one of these cases in the year 1885, when I found it necessary, after dividing some small bands transversely, to pass the knife horizontally under the skin and divide numerous bands passing upwards towards the skin. The appearances in this case are well represented in Fig. 6. In the little finger there is an appearance in many cases of a contracted lateral band of fascia, generally on the outer side, but I have seen it less distinctly marked on the inner side of the little finger. This lateral band, however, takes a longitudinal direction, as shown in Figs. 2, 3, and 6, and in this respect differs from the contracted lateral band in cases of Dupuytren's finger contraction, and has not the effect of drawing down the first phalanx, which is generally drawn backwards, or hyper-extended. In a few cases the contraction increases to a severe degree, so that the second and third phalanges become bent upon the first at a right angle, without any appearance of a lateral band.

Opportunities of demonstrating by dissection the anatomical conditions in these cases must very seldom occur, but my colleague Mr. Lockwood dissected one which occurred in a young woman, aged 21, in whom both little fingers had been contracted

from childhood, and in consequence of the increase of contraction during the last two years she was anxious to have the little finger removed, an operation which Mr. Lockwood performed, having first tried a subcutaneous division of the fascia without benefit. The specimen was exhibited to the Pathological Society on November 17th, 1885, and the report in the Transactions of the Society for 1886 states "it was found that the band in question consisted of a thickening of the digital fascia opposite the flexor aspect of the proximal inter-phalangeal joint. Except that it was thickened and shortened, the fascia was perfectly natural." The specimen is in the Museum of St. Bartholomew's Hospital, No. 1203, B. This period of increase, or the third stage, as I have described it, seems to occur more frequently in young ladies of a neurotic temperament, simultaneously with some loss of health from overwork at school, or natural inclination to study, independently of any constitutional tendency to gout or rheumatism, although I have known parents ascribe it to rheumatism and residence in cold and damp situations.

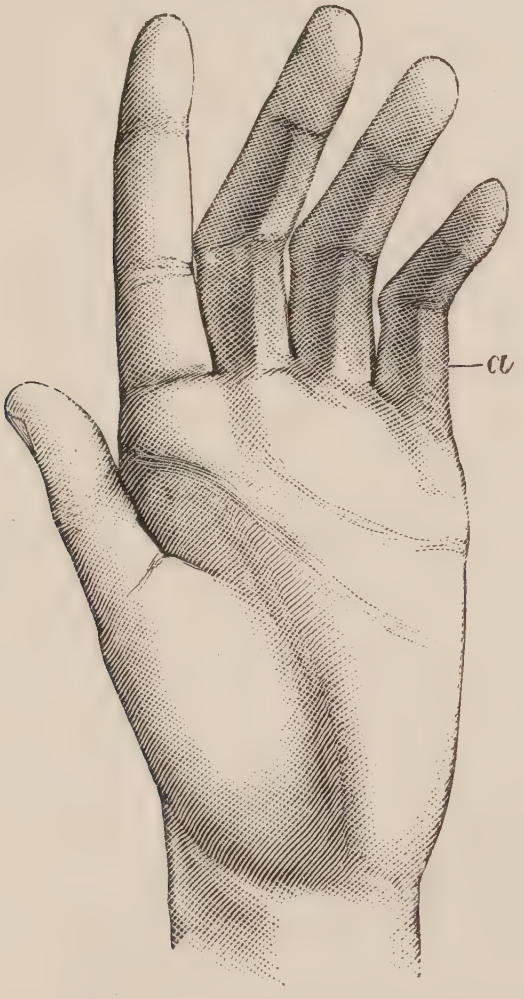
Etiology.

No satisfactory theory of causation of this peculiar form of finger contraction has yet been offered. It used to be thought to depend upon a congenital deficiency of the skin in the direction of the length of the finger—*i.e.*, an inclination to a web in the length of the finger. My former colleague Mr. R. W. Tamplin, the late Senior Surgeon of the Royal Orthopædic Hospital, in his lecture on "Contraction of the Fingers," observes:* "In the congenital form I have not witnessed a case that arose from contraction of the muscles. The cases that I have as yet seen have been occasioned by a deficiency of skin extending from the extremity of the metacarpal bones to the last phalanx (*vide* Fig. 54)." This, by the way, is a good illustration of the ordinary form of congenital contraction, in the third stage, in a young adult, chiefly affecting the little finger; but the other three fingers are also somewhat flexed. It is remarkable that in the same lecture two excellent illustrations are given (Figs. 60 and 61) of a case which Mr. Tamplin considered to be a non-congenital contraction of the skin

* 'Lectures on the Nature and Treatment of Deformities,' by R. W. Tamplin. London: Longmans and Co., 1846.

n the length of the fingers in an adult (see Fig. 5), but I have not the least doubt that this was a congenital case, with fascial and

Fig. 5.



Case recorded by Tamplin as "non-congenital contraction of the skin."*

skin contraction coming on at a late period; one of the exceptional class of cases I have described as reaching the third stage of this form of contraction, and an exact counterpart of this case, may be seen illustrated in Fig. 4. Against this theory it may be urged that there is no appearance of congenital skin contraction, or the formation of a longitudinal web in the finger, when seen in early childhood. I have casts taken from young children that show the transverse creases on the palmar aspect of the phalangeal articula-

* In Fig. 5 the front and side views represented in Figs. 60 and 61, Tamplin, are combined to show the finger contraction as well as the central bands of fascia in front of the phalanges.

tions as well defined as they naturally should be. At a later period, when the second stage—viz., that of confirmed contraction —has been reached, the skin appears to be short, and a longitudinal web becomes apparent when an attempt is made to straighten the finger. It is, I think, from cases in this stage that the opinion must have been formed, but I have no doubt that the skin contraction is a secondary affection, and with it also the terminal cutaneous filaments of the fascia become involved. The only defect which is apparent in the first stage is a dropping of the second and third phalanges (shown in Fig. 1), apparently from some failure of the power of extension, as in the first stage of "hammer toe," with which it is frequently associated; but whether this is due to any failure or imperfect development of the lumbricales and interossei muscles, whose special office it is to aid in the power of extending the second and third phalanges, I am unable to say. Should this be the case, it is worthy of remark that the power of extending the second and third phalanges is not permanently lost through life. If we examine a case in the second or third stage by fixing the first phalanx in the flexed position (as when the hand is clenched), and then ask the patient to extend the second and third phalanges, it will be found that he is able to do this, although the power is always feeble—in part no doubt from disuse. The patient, aged 29 years, whose hands are represented in Figs. 2 and 3, has this power to a very useful extent as a wood engraver.

From the description given above, it will also be at once apparent that this congenital form of finger contraction is not in any way allied to Dupuytren's finger contraction, from which it differs in its etiology and general pathology; in being a congenital affection, whilst Dupuytren's contraction belongs to adult life; in affecting chiefly the female instead of the male sex; and also in its anatomical characters, and difference in the structures involved, as neither the central portion of the palmar fascia nor its lateral digital prolongations are ever involved in the congenital form. The first phalanx is therefore never drawn down, but as a rule is hyper-extended. With these differences are also corresponding differences in the principles of treatment.

Association with Hammer Toe.

For many years I have observed that congenital contraction of

the fingers is frequently associated with hammer toe, occurring either in the same individual or in members of the same family. This occurs too frequently to be merely accidental; but what the relation of the association may be I am unable to say. It is, however remarkable that these two affections have in the early stage their most prominent feature in common, viz., a defective power of extending the second and third phalanges, and this occurs equally in the little finger and the hammer toe, usually the second but sometimes the third toe. It would in this stage be correct to speak of them as a "dropped finger" and a "dropped toe." In one family I am now attending there are four cases of congenital finger contraction (two sons and two daughters) and two cases of hammer toe; one of these, with hammer toe in both feet, is associated with a slight degree of finger contraction in both hands. This occurs in one of the daughters, aged 17; the other hammer toe case occurs in the father. It is not known that either of these affections occurred in the previous generation. In another family I attended many years ago three daughters suffered from congenital contraction of the fingers, and one from hammer toe. I have casts of two of these cases, and one is represented in Fig. 4. The elder brother suffered from a severe degree of hammer toe; he had been operated upon, without benefit, by division of the flexor tendons before he came under my care in January, 1878, but I completely cured the case by division of the lateral ligaments. He was an officer in the Foot Guards, and it seriously interfered with his military duties. I quoted this case in the paper on the Treatment of Hammer Toe I read before this Society on March 19th, 1888.* Both these affections were known to be hereditary in the family. The assistant to the artist who has prepared the illustrations to this paper, a young man, aged 29, happens to have two contracted little fingers, represented in Figs. 2 and 3; and, observing this recently, I asked him about any case of hammer toe occurring in his family, when he told me that he had two hammer toes. His father also had two contracted little fingers, but he did not know whether he had hammer toe. Both little fingers are in the second stage, with the appearance of a lateral web on the outer side in the right finger, and on the inner side in the left finger, as shown in Figs. 2 and 3. He finds no inconvenience from

* 'Proceedings of the Medical Society of London,' vol. xi, 1888, p. 252.

the contraction; but, on the contrary, thinks the contracted little fingers rather more useful than straight ones in holding large flat-headed tools used in wood engraving, and thought they might be called the "wood engravers' little fingers." This association of congenital contraction of the fingers with hammer toe I have seen in many other instances.

Heredity.

The hereditary character of the congenital contractions of the fingers, of which several examples have already been mentioned, appears to be well known, but so lightly regarded that medical attention is seldom directed to them; doubtless, because in a considerable number of cases spontaneous arrest takes place, and the contraction does not advance to a degree which would cause inconvenience. Sometimes, however, young children are brought to us in consequence of the contraction in other members of the family having given rise to some trouble. In the case of Miss Y——, aged 3 years (see Fig. 1), it was well known to have occurred in several members of the father's family. This child had congenital contraction of both little fingers, and was brought from India in the year 1854, as it was thought that some operation might be performed. The late Mr. Joseph Henry Green was consulted, together with Mr. William Freeman, of Spring Gardens. They suggested that I should see the case before any decision was arrived at. Upon examining the case at another consultation, I found there was no evidence of contraction either of the tendons or skin, and the little finger in each hand could easily be restored to the straight position, though the second and third phalanges immediately dropped again. My opinion was therefore against any operation, and in favour only of a retentive finger splint, which was worn day and night for a lengthened period, and at night only for several years. This child was one of a large family, consisting of eight girls and five boys; all the girls are said to have suffered from finger contraction in various degrees, and two of the boys had also contraction of the little fingers, but not in a severe degree. After losing sight of this patient for many years, I had the opportunity of seeing her and examining the hands again in June, 1890. She is about 38 years of age. The little fingers are not quite straight, the second and third phalanges being a little bent, more especially the third phalanx, but this can be easily restored to a

nearly straight position; the voluntary extension power is, however, feeble. It is satisfactory to find that the fingers have not passed into any confirmed contraction, and there is no appearance of a lateral cutaneous web. None of the other fingers have become involved. She is able to stretch an octave with the ring instead of the little finger, but she can use this in playing. There is one remarkable feature in this case which I have not seen in any other instance, viz., that both the little finger and thumb in each hand have been somewhat arrested in their development, and are now very perceptibly shorter than they ought to be, evidently showing some developmental defect.

Treatment.

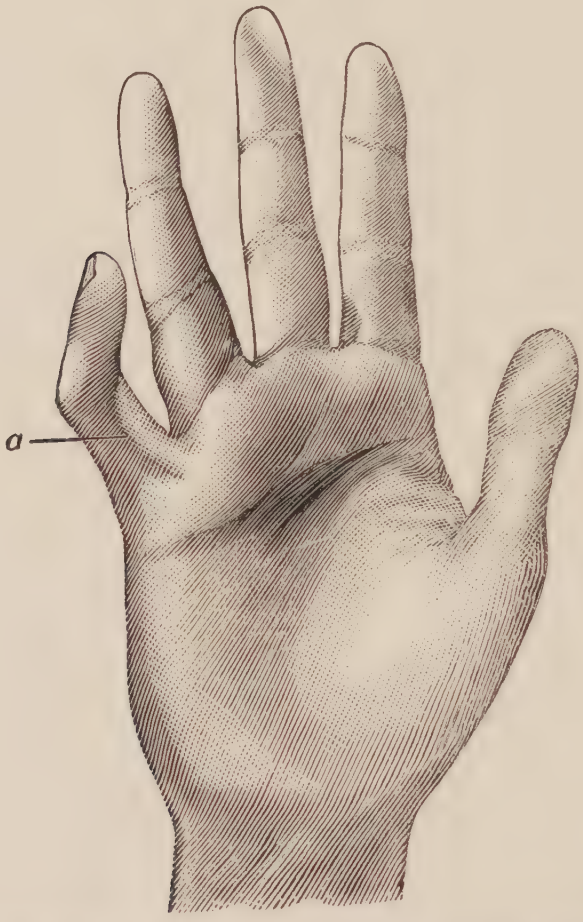
The treatment of the congenital form of finger contraction must be described in reference to the three stages through which this affection may pass, though it only reaches the later stages in a certain proportion of cases.

In the *first stage*, in which the little finger only is affected, the treatment is essentially preventive, the object being to prevent the dropped finger falling into a stage of confirmed contraction with all its inconveniences. This can only be done by the child wearing a light retentive metal splint for a lengthened period, at first day and night, or for a considerable portion of the day, and at night probably for two or three years, or perhaps longer. The finger and hand should be well rubbed and exercised three times a day. The retentive splint may be like either that represented in Fig. 9, or, in a more simple form, a piece of whalebone or steel may be worn inside the finger of a glove.

In the *second stage*—viz., that of confirmed contraction, also generally confined to the little finger, but seldom reached under 10 years of age—the object of treatment is to overcome the contraction, and restore the finger to the straight position. Here, then, the question may be asked whether this should be accomplished by long-continued mechanical extension alone; or by the division of any bands of fascia that can be detected, followed by mechanical extension? For may years I followed the teaching of my predecessors, and opposed the idea of any good being done by operation; but, in May, 1885, my friend Dr. Maughan brought a lady, Miss D——, aged 23, to me, with both little fingers in the second stage of congenital contraction; and the right, represented

in Fig. 6, was so unsightly, and its usefulness so much impaired, that she was anxious to have it straightened if possible. A band of fascia could be distinctly traced beneath the skin, and this I decided to divide before commencing the extension. The prominent contracted band is well shown in Fig. 6. A marked im-

FIG. 6.

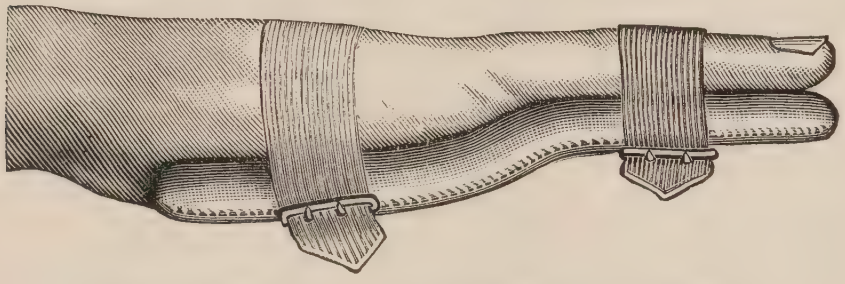


Congenital contraction of little finger in the second stage, with a contracted band of fascia, *a*, near to the median line.

provement followed the division of this band, as well as a number of short contracted bands passing upwards towards the skin. Mechanical extension was then carried on for two or three months, and the finger brought into a straight position. A retentive splint was afterwards worn at night for a long time, and a very satisfactory result was obtained, so that since then I have always adopted this plan in suitable cases. In many cases of congenital finger contraction in the second stage, beneath the prominent cutaneous web, which frequently runs along the margin of the little finger,

one or more small but distinct bands of fascia may be felt, running longitudinally in the direction of the web. These should be divided by the point of the smallest fascia knife, cutting the band or bands transversely; but further relief to the contraction will be gained by cutting horizontally under the skin, and dividing numerous small but tense bands of fascia, evidently the terminal branches passing through the skin. Two or three punctures will probably be required opposite the first phalanx, and two or three opposite the second. Small compresses and plaster should be applied, and the finger bandaged to a flexible metal splint, well padded and applied so as to make some extension, as shown in Fig. 7, but the extension should at first be less than represented.

Fig. 7.

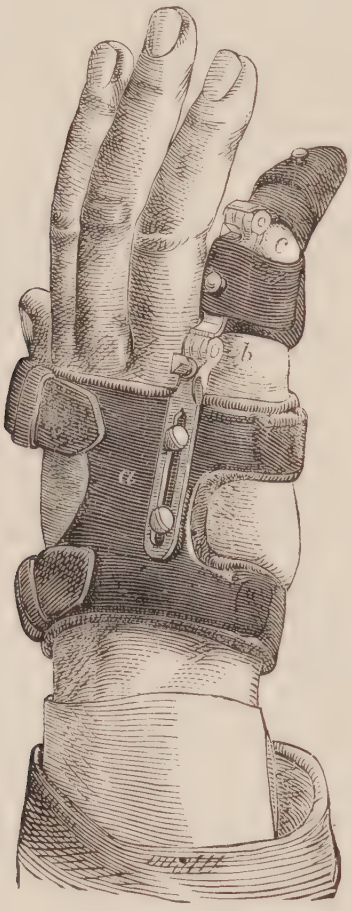


Padded metal splint, as applied after the operation.

On the third day an instrument for making extension should be put on, and the stretching process commenced and carried out as rapidly as it can be borne without producing pain or pressure sores. The principle in all fascia divisions is that of immediate extension, but in practice this cannot always be carried out. The instrument I prefer is the same as that used for Dupuytren's finger contraction, shown in Fig. 8, consisting of a dorsal plate from which levers are carried along the fingers, and these are moved by rack-and-pinion joints corresponding to the phalangeal articulations. The patient may be taught to a great extent to manage this in a few days so as either to release or increase the extension, as it can be borne without pain, but at first the surgeon should regulate this daily. This extension instrument must be worn day and night for a period varying from three to six months, as not only the skin but the deep ligamentous structures yield very slowly to extension, and would quickly relapse if the extension was discontinued too early; but it may be taken off daily for the purpose of washing

and a little exercise. When the extension is complete, the instrument may be gradually discontinued. A simple form of metal retentive splint applied on the palmar aspect, like that used

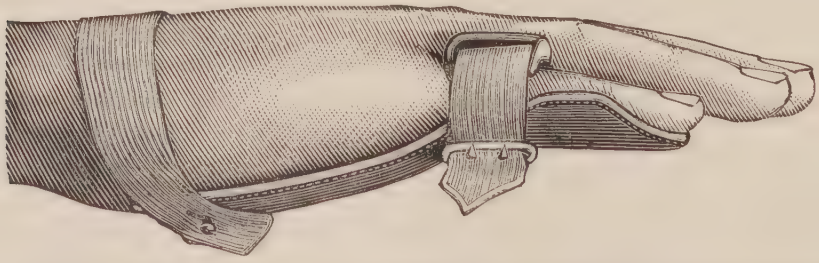
Fig. 8.



Extension instrument, with rack-and-pinion movements, to be used three days after the operation.

for Dupuytren's contraction, as represented in Fig. 9, may be worn for a short time with slight intermissions, and then at night only. This must be continued for many months, till the fingers

Fig. 9.

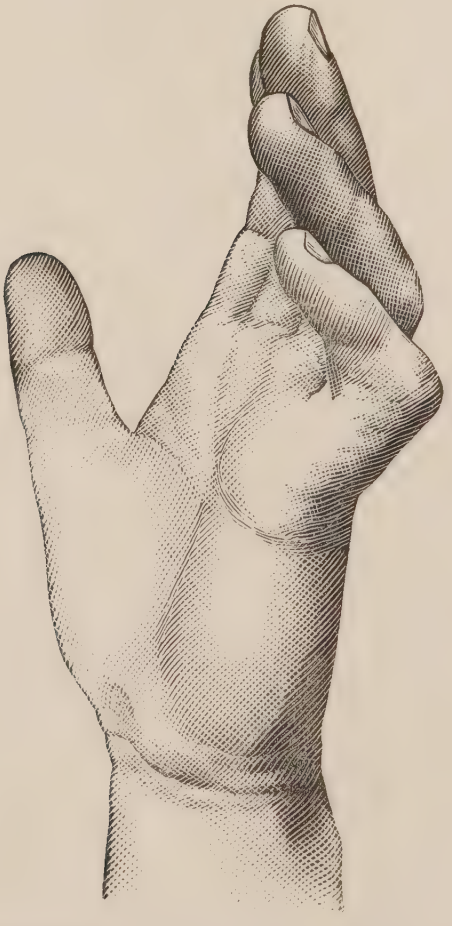


Metal retentive splint, to be used when extension is complete.

during the day's use show no disposition to bend, and the muscles as well as all the tissues involved have become accustomed to the straight position.

In the *third stage*—in which, in addition to the little finger, the middle and ring fingers are always involved, and occasionally also the index finger is somewhat contracted, these fingers being drawn down in some cases at a late period of increase, say, from 16 to 18 or 20 years of age—the operative and mechanical treatment are both essentially the same as that already described in the second stage. All the fingers involved should be operated upon at the same time, a careful division being made of the longitudinal bands running on either side of the median line of the first and second phalanges, and the terminal cutaneous fibres connected with them, which seem to give the appearance of an elongated square, flattened band in the centre of each phalanx. The after mechanical

Fig. 10.



Congenital contraction of all the fingers in the third stage, in lady aged 23. Before operation.

treatment is precisely similar to that described in the second stage, but the extension instrument adapted to all the fingers should be worn night and day for a period of from three to six months, according to the severity of the case and the age of the patient. Several cases have been successfully treated in the third stage.

In one of these cases occurring in a young lady aged 23, in whom both hands were affected (and the left hand is represented in Fig. 10), this treatment was rigidly carried out for six months, the extension instrument being worn night and day, but removed for washing and exercises every day, and the retentive metal splint is still used at night. The photograph showing the improvement

Fig. 11.

From the same case as Fig. 10, eight months after operation, showing the complete removal of all contraction.

after treatment, from which Fig. 11 is copied, was taken by my colleague Mr. Macready (who has assisted me in many of these operations, and has carefully studied both the Dupuytren and the

congenital finger contraction), about eight months after the operation, in April, 1890, and up to the present time (May 1891) the improvement has been fully maintained, and she has full use of the fingers in playing the piano, &c. The right hand, somewhat less contracted than the left, is now undergoing similar treatment, which I have no doubt will be equally successful.

In conclusion, my experience clearly proves that when taken in the first stage contraction may be prevented by mechanical means alone; and that either in the second stage, when the contraction is confined to the little finger, and has become spontaneously arrested with deformity, or in the third stage, when contraction increases from 16 to 20 years of age, and all the fingers become involved, a complete cure may be obtained by operation followed by a long course of mechanical treatment, *i.e.*, wearing an extension apparatus day and night for a period of from three to six months after the operation, and a metal retentive splint, adapted to all the fingers, afterwards at night for some months. We may, therefore, consider that this congenital contraction of the fingers, like Dupuytren's contraction previously described, has now been brought fairly under the control of the surgeon.

Mr. Macready said that he could corroborate the frequency of the relation between bent fingers and bent toes that had been detected by Mr. Adams. The two deformities often co-existed in the same person and existed apart in different persons of the same family. At times a bent finger in the parent was followed by hammer toe in the child, or conversely. But the descent of a bent finger from parent to child was the most common inheritance, and was met with in 60 per cent. of his cases. This high percentage was in accordance with Dr. Ogle's belief that these bent fingers are the most hereditary of hereditary maladies. In twenty cases that had been under his (Mr. Macready's) observation, the affection appeared in two forms. In most of the cases the little finger was flexed at the first phalangeal joint. In two cases all the fingers were simply arched. In about half the cases these two deformities were associated in the same hand, so that the little finger was flexed and the other fingers were arched. He had not observed that the malady was progressive, as was held by Mr. Adams. He thought that probably no marked alteration took place in the degree of the deformity with the lapse of time, especially as there were indications that the affection was due to arrest of development. It was already admitted to be congenital and hereditary, and he would call to mind that, in malformations of the hand notoriously due to arrest of development, the fingers, if present, were almost always flexed, and flexed, as in the generality of these cases, at the first phalangeal joint.

Mr. Spencer Watson suggested, as a theory consonant with the acknowledged heredity of Mr. Adams' "crooked little finger" and its similarity with the crooked little toe in ordinary Europeans, that the

ancestors of persons with "crooked little fingers" were probably people of literary pursuits, or occupied in such a way that the *prehensile* functions of the hand had been very slightly exercised, and that in the course of successive generations this function had so dwindled that it resulted in an atrophied and distorted condition of the fingers, the little finger being, however, the first to show its deficiency ; that this atrophy of the little finger was analogous to that of the little toe in Europeans as compared with the highly developed toes in the Quadrumana and certain races of man who go barefoot, and in whom the foot may be regarded as a *prehensile* organ.

Mr. Adams, in reply, said he agreed that both the hammer toe and the congenital finger contractions were more or less of neurotic origin, and the want of power of extending the second and third phalanges in both affections, in the first stage, pointed to some defect of innervation. No active or passive contracting force could be detected. Whether any developmental defect existed, Mr. Adams was unable to say. In cases in which the third stage was reached, and in addition to the little finger, the other three fingers became contracted, these occurred chiefly in young ladies of a neurotic temperament between the ages of 16 and 20 years. In these cases contracted longitudinal bands of fascia became developed, and as a passive contractile force required division. With regard to Mr. Spencer Watson's remark, that this contraction may be due to the gradual loss of the hand as a prehensile organ, he (Mr. Adams) observed that no such physiological views had occurred to him. Mr. Macready bore testimony to the relationship between the bent fingers and bent toes which he had found to occur in 60 per cent. of the cases which had come under his observation. With regard to Mr. Macready's observations on the relative value of the operative and mechanical treatment, he (Mr. Adams) had relied upon mechanical means alone for many years, following his colleagues Mr. Tamplin and Mr. Lonsdale, without very satisfactory results ; but since he had divided the longitudinal bands, as well as the terminal cutaneous branches, previous to mechanical extension, the cases had been completely cured in about four or six months.

SOME CASES IN WHICH A NON-MALIGNANT COMMUNICATION EXISTED BETWEEN THE BLADDER AND THE INTESTINES.

By Reginald Harrison, F.R.C.S.

I wish to bring before the Society some kinds of vesico-intestinal fistula which have come under my observation, with the view of eliciting opinions as to what is best to be done for them. In this communication I shall exclude those varieties due either directly or indirectly to malignant disease, and to the ordinary forms of stricture, as well as those rare instances of congenital communica-

tions between the urinary and intestinal viscera. I will introduce the subject by giving a brief account of the following cases :—

Case 1.—A man, aged 58. Ten years before I saw him he was laid up for some weeks with acute pain in the lumbar region, which resulted in the discharge of pus into the bladder and its escape with the urine. On the sudden appearance of purulent urine the pain in the back ceased and he rapidly convalesced. It was suggested to him at the time that the abscess was probably connected with the kidney, but I could now find no evidence of this. On going about again, after this attack, he noticed the sputtering of air at the close of micturition, and this at times has continued ever since, but has caused him no further inconvenience. The particular symptom about which I was consulted was the occasional discharge of a colourless fluid from the rectum independently of defæcation. I did not see any of the fluid, but it was sufficient when it occurred to moisten the linen, and thus to make the patient uncomfortable. On examination of the rectum with the finger and speculum I could not detect anything abnormal except a little irregular enlargement of the prostate, but not such as is usually met with in ordinary hypertrophy of this part. It will be noted that the patient was rather below the hypertrophic age.

The urine was normal in composition, but contained a slight deposit which microscopic examination showed to contain elements of muscular tissue and vegetable fibre.

In this instance the sputtering of air at the close of micturition, the occasional discharge of what I believe to be urine from the rectum, and the presence of elements of food in the urine pointed to the probable existence for at least ten years of a communication between the bladder and intestines, dating from the pain in the back and the sudden discharge of pus into the bladder referred to. I would add that the sputtering of air, together with some irritability of the bladder, is always increased when there is diarrhœa. He has never noticed any discoloration of the urine with fæcal matter, a circumstance which may be due to the fineness of the sinus. I believe the patient is, with this exception, still in the enjoyment of good health.

Case 2.—I saw a man in 1887, aged 50, who two years previously suffered from some obscure abdominal symptoms in the region of the bladder, attended with acute pain, since which he has noticed fæculent matter in the urine and the discharge of air at the close of micturition. Examination of the rectum disclosed nothing worthy of note further than that I was able by pressing on the prostate and posterior wall of the bladder to make urine mixed with fæculent matter exude from the orifice of the penis. In all other respects this patient seemed to enjoy excellent health, and there was no evidence pointing to malignancy. I have seen him on two or three occasions since in consequence of his suffering from an acute paroxysm of pain over the region of the bladder. Each time I found the pain was due to the blocking of the prostatic urethra with a hardened nodule of fæces and the rapid distension of the bladder with air.

Immediate relief was given by passing a catheter, which afforded vent to some fæculent urine and a large amount of pent-up gas. This patient's symptoms are always intensified when diarrhœa is present. Though much annoyed at times by these local symptoms, he continues, I understand, to enjoy good health and to lead an active life.

Case 3.—A man, aged 61, was brought to me suffering from an irritable bladder and occasional attacks of hæmaturia extending over a period of seven years. The intervals between the attacks of hæmaturia were very variable ; the bleeding usually continued for three days, and the last attack was four months before I saw him. Latterly he has had painful sensations when making water. The hæmaturia is invariably preceded by pain in the back. Micturition is unnaturally frequent both by day and night. During the last three months he has noticed that air bubbles were passed frequently during micturition, and particularly at the close of the act. On one occasion he appears to have thought that fæces were present in the urine, but he never detected any odour of this kind. There was no prostatic enlargement. He was ordered half-grain cocaine suppositories to allay irritation. The urine was practically normal, and no deposit of any kind was detected by the microscope. Nine months after I saw the patient his medical man wrote to say that for some time he was the better for the treatment, when the old distress and frequent desire to micturate returned. The passage of gas by the urethra had also become of more frequent occurrence. Urine a light mahogany colour, depositing about one-twentieth. Acid. Odour, strong ; somewhat like fœtid pus. This patient has recently died, and his medical man informs me that kidney disease was the probable cause of death. There was no evidence of malignancy.

The occasional attacks of hæmaturia render the case somewhat different from the two preceding ones, though like them in that there was evidence of a communication between the bladder and some part of the intestinal tract. Whether the blood proceeded from the kidneys or the sinus it is impossible to say. The fact that the patient eventually died apparently of renal disease is not unlikely to be connected with the putrefaction of the contents of the bladder by admixture with fæcal matter, and the regurgitation of the products of decomposition, either gaseous or liquid, along the ureters towards the kidneys.

Case 4.—A man, aged about 60. His last illness commenced a short time before I saw him with intense pain down the course of the right sciatic nerve which lasted for rather more than a week : then retention of urine supervened from enlarged prostate, in addition to some urethral stricture which necessitated daily catheterism. In the course of a few days the pain in the back and leg entirely ceased, coincident with the appearance of pus in considerable quantity in the urine. The urine as soon as it became purulent was rendered most offensive, in spite of the bladder being thoroughly washed out with suitable disinfectants. The presence of some degree of urethral stricture necessitated the use of

a smaller catheter than otherwise would have been preferred. He appears to have experienced some difficulty in urinating for several years, a circumstance which was probably due to the contraction in the urethra. He had not lost flesh, and there was no evidence that seemed to point to the presence of malignant disease. He had always led an active life. The urine continued to be horribly offensive, as large quantities of gas and fæces were constantly mixed and discharged with it in spite of all the means that were used. He remained much in this way for about two months after I first saw him, and then lapsed into a comatose state from which he never rallied. He was unwilling to submit to such operative measures as were proposed for his relief. The patient stated that when he was about 20 years of age he suffered from an abdominal abscess of some kind, when it was believed that a communication had taken place between the bladder and intestines, as pus, and what he thought was fæculent matter, had been at times discharged with the urine, but never sufficiently to cause him inconvenience. He had occasionally passed air at the close of micturition, which he likened to the expiring efforts of a siphon soda-water bottle. In all other respects he appears to have enjoyed good health. His urine was on several occasions kindly examined for me by Sir William Roberts, and showed unmistakable evidence of intestinal contamination.

I am indebted to Mr. Weston, the surgeon with whom I saw the case, for the following particulars of the autopsy. The examination was only of a partial character. There was an opening near to the top of the bladder, looking as though it had always existed, lined with mucous membrane, not patent but contracted, and much resembling a miniature anus. No. 8 catheter passed easily through it into a cavity pretty well surrounding the descending colon. This cavity, or rather the walls of it, were firmly adherent to the surrounding parts and were with great difficulty separated. The cavity contained no fæcal matter, but was rough from inflammatory products and rotten in appearance. There were several openings from this cavity into the intestine, but in the hurry of making the examination the intestinal connections were a good deal disturbed. The colon took an unusual course ; instead of passing down into the left loin it curved across the lower part of the abdomen in front of the small intestines and disappeared a little to the right side and under the bladder to its termination in the anus.

I will now briefly narrate the particulars of another example which has elsewhere* been more fully recorded, inasmuch as it seems to throw some light on the pathology of the subject.

Case 5.—A man, about 50 years of age, whom I was asked to see in 1882. For six weeks he had noticed that his stools were not formed, and were more or less loose. Three weeks previously his bladder became extremely irritable, and he often experienced severe pain in the lower part of the abdomen. He also observed that quite recently his urine contained a considerable quantity of pus, which he could only void with much straining. Shortly after matter appeared in the urine, he noticed that the act of micturition terminated with a sort of fizzing sound and that bubbles of air were expelled. On examination with the hand there was a considerable swelling over the fundus of the bladder, hard

* 'Liverpool Med. Chir. Journ.,' Jan., 1884.

and tolerably well defined, about as large as a cricket ball. I passed a sound into the bladder, but could detect no stone nor anything abnormal to the touch. An examination of the urine showed a deposit consisting almost entirely of pus. In the course of a month the patient's condition considerably improved, though pus, air, and a slight amount of fæcal matter were present in the urine. About this time the patient passed by the urethra, after some trouble, a mass, the size and shape of a horse-bean, which he thought was a stone. Fortunately this substance was preserved for examination, and proved to be a fractured portion of the expanded end of a rabbit's leg bone. From this date all the acute bladder symptoms subsided, and though for the remainder of his life the patient at times, particularly when the bowels were loose, passed small quantities of air and fæces with the urine, his general health in no way suffered from what has been described. There could be no doubt that the sharp piece of bone made its way from the gut into the bladder and left a small sinuous tract between these viscera which never completely closed.

In reviewing cases of this kind, more particularly in reference to the question of treatment, we can divide them into two classes: (1) those where the inconvenience resulting is so slight as would hardly warrant the adoption of any important surgical interference; and (2) those where the inconvenience is an increasing one, either by a gradual process or by the intercurrence of acute symptoms, or where the condition of the bladder by decomposition of the urine becomes such as to seriously jeopardise the health of other parts of the urinary apparatus, especially the kidneys. I think it will be found that the cases quoted illustrate in some degree these various contingencies.

That persons live for many years and enjoy good health who are liable to the escape of gas and fæces in this way I do not think there can be any doubt of. I have had the opportunity of examining some of those cases which have been described under the term pneumaturia, implying that the urine or other proper secretion connected with the urinary apparatus had undergone some form of change by which gas was evolved and either expelled or temporarily retained. With one exception,* I have no hesitation in stating that they were all instances of vesico-intestinal fistula. I feel doubtful now as to the true character of the case which certainly seemed to me to form the exception, especially since I have been opening the bladder above the pubes and seen the ease with which air enters this viscus and is retained even when careful catheterism has been employed. It has been stated that air is met with in the urine in certain cases of glycosuria, and

* 'Surgical Disorders of the Urinary Organs.' By Reginald Harrison. 3rd Ed., p. 57.

Dr. Guiard proposed the term "diabetic pneumaturia."* I am not aware, however, that this observation has been confirmed. Cases where the communication between the bowels and the bladder is evidently small, and is not increasing, may be kept in good health by careful attention to the digestion and by restraining a tendency to diarrhœa, which invariably aggravates any special symptoms that they may present.

Passing to the second class of cases, where the communication becomes gradually or suddenly enlarged, or where the urine is rendered so foul as to jeopardise the kidneys as well as other portions of the urinary tract, it is clear that some mechanical expedient in the shape of an operation must be considered. To do any permanent good under such circumstances it is obvious that the flow of fæces into the bladder should be prevented by causing them to escape by an artificial opening above the point where they are poured into the bladder, and thus to give the false passage a chance of contracting and closing. This is a proposition which on paper is an easy one to make, but in practice may be a difficult one to decide upon. And the difficulties in this respect are, in a large measure, due to the absence of those circumstances which may be said to determine us when we know almost for a certainty that malignant disease is connected with the lesion. It must not be overlooked that in many cases of vesico-intestinal fistula which appear to have taken their origin in an inflammatory condition, such as in several recorded by Mr. Harrison Cripps† in his interesting monograph on this subject, the viscus involved with the bladder was some portion of the small intestine. Taking, however, everything into consideration and the probability of our being able to distinguish when the latter is implicated, on the necessity arising, it is the safest practice to make the opening as high up in the large bowel as possible, and this will lead to the selection of some portion of the right or ascending colon. In one of the instances related I was much disposed, considering the condition of the patient, the state of his bladder, and the uncertainty as to where the false opening was, to give that immediate and safe relief which a supra-pubic opening into the bladder could have afforded and be guided as to the future by what this would have enabled the eye as well as the finger to discover.

* 'La France Médicale,' vol. i, 1883.

† 'On Passage of Air and Fæces by Bladder,' 1888.

Mr. Harrison Cripps referred to his monograph, wherein he had collected sixty-three cases. Only eight were malignant, the remainder being inflammatory; many from abcesses between the intestine and bladder, and others secondary to ulceration and stricture. In twenty-five cases the communication was between the upper rectum and the bladder; of the remainder, two-thirds were with the large gut and one-third with the small. If the communication were very low down, digital examination might detect it, but if high up, one was dependent upon the symptoms. In cases of communication with the small gut, the fæces found in the urine were to a large extent undigested, and were always present. In the large gut cases the material presented well-marked fæcal characters, and was not always present; but it was usually increased by diarrhœa. and if milk were injected into the rectum it usually speedily reappeared in the urine.

Mr. Reginald Harrison, in replying, said that when the urine was rendered constantly putrid by admixture with fæces in the bladder there was, he believed, a considerable risk of kidney mischief, including that suppurative condition known as surgical kidney, being set up. The same was seen occasionally in advanced prostatic disease where the urine became decomposed and ammoniacal. He thought this circumstance alone was sufficient to warrant an attempt being made by operation to render a mixture of fæces and air taking place in the bladder impossible.

December 15th, 1890.

REMARKS ON THE BARKING COUGH OF PUBERTY (CYNOBEX HEBETIS).*

By Sir Andrew Clark, Bart., M.D., LL.D., F.R.S.

Some thirty years ago Dr. Brown, of Rochester, sent me for examination a boy of 14 years of age, the subject, for several months, of an incoercible convulsive cough. The patient belonged to a nervous family; he was himself nervous, and, whilst under examination, was odd in movements and restless in manner. Small, thin, pale, and fair, but quick in movement and in response, he exhibited all the characteristics of extreme "delicacy." Careful examination brought to light no sign of special structural disease, and at the end of the examination I found myself no further than at the beginning, and could say of the patient only that he was a very delicate and nervous boy. But when preparing

* Suggested by my friend Dr. Rutherford, Head Master of Westminster.

to leave the room, the lad, startled by a sudden noise, began to cough, and had a paroxysm of coughing which lasted for over a minute. The paroxysm consisted of a close succession of loud, dry, clanging, convulsive coughs, varying in intensity and duration, broken into irregular bars and phrases, and resembling at one time the barking and at another the howling of a dog. During the continuance of the paroxysm the patient appeared to be much distressed; his face was swollen, and faintly livid; the eyeballs became prominent and congested; the body, bent forwards, was shaken by the violence of the coughing, and the hands, crossed upon the chest, convulsively clutched his clothes. At the close of the paroxysm the patient appeared to be a little dazed, and was somewhat giddy. Recovering himself in a few minutes, he passed a quantity of limpid urine, and the storm was at an end. The malady in this patient lasted over thirteen months, and then slowly subsided. No ill effects followed; but I, some years afterwards, learned that the patient was still nervous, and that a dry, convulsive cough was readily provoked by trifling colds.

In 1868 I was consulted by a clergyman residing in the east of London, about his son, who was supposed to have some grave but obscure disease of the lungs. The boy was 13 years of age, well nourished, fair, with all the characteristics of a nervous temperament, and the history of some minor nervous troubles. The parents and their relatives were nervous; and the father, subject to involuntary movements of the right arm and hand, had occasionally found difficulty in writing. The boy's malady consisted in daily recurring violent paroxysms of convulsive barking rhythmic cough, which produced considerable exhaustion and distress, and had provoked, on one occasion, an involuntary discharge of urine, and on several occasions vertigo. The affection had begun six months before, and latterly there had been some loss of flesh, strength, and colour. No school would keep the patient; the members of his family were distracted, and the father was despairing and perplexed. Repeated examinations of the patient, by myself and by others, failed to elicit evidence of the existence of structural disease in any part of the body; all that I could discover was a somewhat bizarre condition of the nervous system, and all that I could say was that the malady was nervous, and that it was devoid of permanent peril to life or to health. In this case the affection lasted nearly three years; but then recovery was complete; and

through a careful regimen, strict discipline, and change to the seaside, the bizarre condition of the nervous system disappeared.

In a third case, the daughter of a clergyman, about whom I was consulted in 1875, the attacks were of the same general character; but occasionally they became so severe as to produce squinting and temporary unconsciousness. This girl was 13 years of age, and her malady had begun with the advent of menstruation a few months before. The offspring of nervous parents, she also was intensely nervous, and exhibited from time to time diverse signs of nervous disorder. The noise made by this patient during her attacks was so loud and so peculiarly distressing to others that, with the exception of the mother, no one would remain in the house during their continuance; and even the mother confessed to me that she felt as if the limits of her own endurance were at hand. After a few months of unsuccessful treatment this patient was cured quite suddenly through the agency of a fright. One day, whilst in a paroxysm of coughing of exceptional mildness, it was proposed to perform an operation upon her throat, and a box of cutting instruments was laid open before her. Without a word she swiftly fled from the room, and managed to keep herself successfully concealed until late at night. When she returned to her mother she had lost her cough, and, three years afterwards, I was told that it had never returned.

It will have been noticed that in all the cases of which I have spoken, the subject of this pubertal cough and also their parents and families were "nervous." But in a case which I saw in 1880 with Mr. Aikin, this was not the case. The parents were healthy, and had come of a healthy stock, and the only signal evidence of nervousness in the patient was the existence of the malady (the convulsive cough) with which he had became affected. The boy was between 14 and 15 years of age, had the build and appearance of health, and never suffered in any serious way until the "breaking of his voice," when his malady began. The attacks in this case were not violent, but they were accompanied by irregular action of the heart and occasionally by dry bronchial râles. The patient recovered within six months, but, twice within the ten years that have elapsed since then, he has had an attack of bronchitis accompanied by a little bronchial spasm.

All the cases to which I have adverted are examples of the severest forms of the malady of which I am speaking, and they

represent the chief varieties of it which have come before me in practice. But the attacks are slight as well as severe, and there are other varieties of it which I have thought unnecessary to notice. Except in degree, the cases so closely resemble each other that from the study of one we may learn the characteristic features of all. Of the four examples which I have brought under notice of this convulsive cough of puberty, three have occurred in boys; and it might excusably be inferred therefrom that this represented the numerical relation of occurrence between the sexes. But it is otherwise. The cases referred to were chosen in illustration of the varieties of the malady; and of the whole number which has come within the range of my experience the greater part was found in girls. Before proceeding to discuss the nature of this malady, it may be well to point out that the cough, which is its characteristic local feature, is not an ordinary nervous cough; that it is not the ordinary cough of local irritation, of related pathological changes, of reflex action, of pneumogastric trouble, or of mere hysterical disorder; but that, possessed of certain distinguishing characteristics, existing always in the same general environment, and occurring almost exclusively within the range of those physical and psychical changes which mark the advent of puberty and accompany the final evolution of sex, it has a well-defined individuality of its own, and claims, even as one of the habit-spasms so admirably described by Gowers, the right of separate consideration and naming. It is true that little, if any, notice of this malady has been taken by previous observers; for, after a search through the chief annals of medical literature, made by Dr. Delépine and myself, there were found, with three exceptions, only distant and obscure allusions to the convulsive or barking cough of puberty. The first notable exception occurs in the sixteenth volume of the 'Transactions of the Clinical Society,' where, in the presidential address delivered by me, a brief description of this malady will be found. The second notable exception will be found at page 491 of the first volume of Sir Morell Mackenzie's 'Manual of Diseases of the Throat and Nose.' There the disease is plainly recognised; but the author's description of it differs in some important points from mine. It is described as occurring in young girls between 16 and 20, of having been met with in boys between 14 and 16, and of having been seen several times in children between 5 and 14 years of age. A third excep-

tion appears at p. 587 of the second volume of the work of Dr. Gowers on 'Diseases of the Nervous System.' Notwithstanding such scant notices as have been found of this malady, I have little doubt that its existence has long been known to practised clinical observers; and I can find no just grounds for regarding it as one of our new diseases.

In proceeding to consider the nature of the convulsive cough of puberty, one must keep in view the extent, variety, and character of the changes which occur in the organism during the final evolution of sex. They are manifold as well as profound; they are central as well as peripheral; they are psychical as well as physical; they are moral as well as mental; and they are accompanied and penetrated by a fresh awakening of the self-consciousness which carries its organism into a new arena of thought and of life. It cannot be doubted that through the procession of changes, so many and so varied, all the ascending nervous hierarchies are actively at work creating new activities, establishing new relations, adjusting new environments, and establishing new control. One obvious result of those excited activities is that the whole central nervous system is brought into a condition of unstable equilibrium, and is ready, in irritable organisms, to explode on small provocations into convulsive action. And when one remembers that peripheral changes are also undergoing concurrent evolution, that one of the principal seats of them is the laryngeal region, and that the nervous hierarchy under which they exist is now perhaps the most active of all, it is not difficult to understand why troubles should occur in this region at this time, and why, when they do occur, we should expect them to be of a nervous character. In those central and peripheral activities we have conditions ripe for disorder, and either autogenetically or deuterogenetically, from inborn disturbances or from extrinsic irritations, we can see, without stretch of vision, how this malady may arise. When, furthermore, we remember that for the most part the family and the personal history of the patient is nervous, that no local changes adequate to the explanation of the malady are to be found in the pharynx, in the larynx, or in the lungs; that the cough and other disturbances of the respiratory movements are rhythmic and paroxysmal; that no structural degenerations follow in the wake of the affection; and that it disappears with, or shortly after, the complete evolution of sex, we cannot doubt—at least, we cannot,

I think, justly doubt—that this convulsive or barking cough of puberty (cynobex hebetis pubertatis tussis canina) is a malady of nervous origin, character, course, and issues. But in how far the inborn changes which engender it are central or in how far peripheral; to what extent they are modified or controlled by extraneous influences; whether they are structural or dynamic; and what may be their exact seat or intimate nature, I refrain at this time from striving to determine.

Concerning the prognosis of this malady, it will be sufficient to say that in all the cases of which I have had any direct knowledge, and they number over twenty, the course, although usually tedious and sometimes prolonged, has ended eventually in complete recovery.

I proceed now to speak of the treatment of this malady. Unfortunately, I am compelled to confess my belief that I have not succeeded by any treatment which I have hitherto employed in shortening, to any great extent, the duration of the disorder. Nevertheless, I am as sure as I can well be about anything of this kind, that by an appropriate regimen, by sedative applications to the interior of the throat, and by the administration of certain internal remedies, I have succeeded, without disturbance of the patient's health, in restraining the cough within harmless limits, in averting secondary evils, in improving the general health, and in guiding the course of the malady to a somewhat speedier issue than otherwise it would have reached. With respect to regimen, I have to observe that in all the cases brought before me the patients were plainly overfed; that, in addition to ordinary meals, there were frequent "interludes" of different kinds of food, mostly preserved; and that alcoholic beverages of some sort or another were freely administered. For my own part, I am unable to believe that the laws of digestion are suspended because an organism is troubled with a malady of this kind, that there is no physiological limit to the administration of food in disease, and that one is not certain to make a bad fire when one keeps putting too many coals upon it. Improvement has almost invariably followed the enforcement of a simple but liberal dietary, arranged into three, or at most four, meals a day, abstinence from alcohol, cold or tepid sponging, warm but not too warm clothing, active out-door exercise, early hours, and general discipline. Poor platitudes these, it may be said. Yes. Plainly so on paper, but not so

in practice, where they can effect so much, and are yet overlooked so often. Of local applications to the interior of the throat only two have done good service. The first is glycerin of borax with oxychlorate of bismuth and morphia,* and the second is the same combination with the substitution of cocaine (10 per cent.) for morphia. These applications should be brushed over the whole interior of the throat after each meal and also at bedtime. In using cocaine, care must be taken in its application to the larynx, which is sometimes provoked by it into dangerous spasm.

Among the internal remedies which I believe to be of use in this malady, there are also two. The one is a syrup of the bromide of quinine and iron, with small doses of arsenic; and the other is a pill of reduced iron, valerianate of zinc, belladonna, and nux. This latter combination, often successful in controlling the cough, must be given in doses slowly increased until the physiological effects of the belladonna become apparent; and then, slightly diminished, the amount administered should be maintained at that level.

There is still another remedy to which, in laryngeal nervous affections, specialists in throat disease attach great importance, and that is a long sea voyage. Undoubtedly it is sometimes successful; but undoubtedly also, in my experience, it oftener fails. Furthermore it is costly, it is difficult to carry out, it interrupts the work of education, and occasionally, in the case of boys, it begets habits of a kind which permanently interfere with a right conduct of the business of life. For my own part, when I now review my experience of the effects of sea voyages upon young invalids, I am more strongly impressed than ever I was before with the necessity of extreme caution in prosecuting them.

But although in all these ways the benefits which we shall be able to confer upon our patients may not appear to be great, there is one way in which perhaps we may be enabled to make them great beyond expectation or expression. We have seen that almost all the subjects of this malady are "nervous"! and from this we know that they tend to become self-conscious, to dwell upon their disorders, to give way to self-distrust, to take interest only in themselves, and at last to fail in all that is useful as well as all that is high in life. Coming at this early time of youth under our instruction and influence, we may help them to strive after self-

* ℞. Glycer. boracis, ʒj.; bismuthi oxychlor., ʒij; morphinæ hydroch., gr. 11½. Fiat applic.

effacement, to aim at some higher ideal than themselves, to find in labour strength, and through suffering tranquillity; and so, transfiguring their lives, to discover therein sacrifice instead of selfishness, and gladness instead of gloom.

Dr. Althaus expressed his high sense of the value of Sir Andrew Clark's paper, but could not agree with the author in considering the particular kind of cough which had been described as unique, or exclusively belonging to the class of cases under consideration. He contended that exactly the same kind of short, dry, loud, rhythmical, convulsive cough, owing to spasm in the adductor muscles of the vocal cords, was apt to occur in the milder form of the laryngeal crises of locomotor ataxy, while in the severer forms of those crises other convulsive symptoms; and as stridulous breathing, involuntary micturition, biting of tongue, the cramps in the limbs, with headache, vertigo, and vomiting, might also be present, some of these latter symptoms having also occurred in one of Sir Andrew Clark's patients, he thought that our views of the pathology of the laryngeal crises of tabes might be made to apply to, and throw light upon, the cases now under consideration. It was well understood that exceedingly severe symptoms of laryngeal spasm might, and did, occur in those crises, without there being any morbid affection of the throat trachea, or other portions of the respiratory organs; while in a case examined by Cruveilhier, wasting of the roots of the pneumogastric nerve in the medulla oblongata had been discovered, showing that the symptoms during life had been dependent upon an unduly excitable condition of the cough-centre, which Kohts, of Strassburg, had localised in the alea cinerea, a little above the situation of the respiratory centre, on the floor of the fourth ventricle. In the case of a hysterical girl which had some time ago been under Dr. Althaus's care, an attack of laryngitis which had taken place some years before appeared to have given rise originally to such an irritable condition of the cough-centre, while this latter state persisted long after every trace of the laryngitis had disappeared, and he suggested that a similar occurrence might have given rise to the convulsive cough in some of Sir Andrew Clark's cases, the cough-centre eventually assuming a degree of autonomy. He also drew attention to the ease with which paroxysms of cough could, under such circumstances, be elicited by irritation of remote portions of the surface of the body, and in hysterical girls chiefly by pressure on the breasts and ovaries, showing the purely nervous character of the affection.

Dr. Gowers expressed his concurrence in the warm appreciation of the paper which had been expressed by the previous speaker, and especially valued it for the manner in which it drew attention to the deeper relations of the symptoms, the significance and study of which are of so great importance. The cough described had come under the speaker's notice in a considerable number of instances, and they seemed to him to fall into three classes:—First, cases in girls who present also various symptoms on the part of the nervous system which we are accustomed to term "hysterical;" and in these cases the cough seemed to be of the same nature. It must be remembered that many of the symptoms of hysteria are to a considerable extent independent of the simple influence of the will, however strongly they may be under the influence of the will when this is strengthened, and almost transformed, by a strong desire. The two other classes occur in boys. In one of these the symptom forms part

of the affection known as "habit-chorea," or "habit-spasm," in which the peculiar movements often change their form and seat from time to time; this cough may alternate with a spasmodic movement in some other part of the body. In the third class of cases it exists in boys as an isolated symptom, and in these Dr. Gowers had found that it was almost always associated with the habit of masturbation, that treatment had little or no influence until this habit was stopped and then the patients rapidly get well whatever treatment was employed. This point was of very great practical importance. The cough was itself, he believed, as characteristic as had been stated in the paper. He would not deny that in some cases of laryngeal crises in locomotor ataxy a noise might be made resembling that in some cases of cough, but certainly in the cases of each disease that had come under his own observation the symptom was quite different. The peculiarity of the cough was the strange barking sound produced, and this differed from all ordinary kinds of cough, so that he thought it correctly described as *sui generis*, and he thought this was all the author of the paper had intended to assert, not that no other condition can possibly be met with in which there was a similar cough. He had not met with any case so severe that the paroxysms produced involuntary evacuations, but otherwise he could testify to the faithfulness of the description given by Sir Andrew Clark. With respect to the cases in hysterical subjects, it was interesting to note that the globus hystericus is a symptom in the region of the function of the respiratory nerves; there was a sense of closure of the throat, and often of suffocation, and the convulsive cough might be regarded as, in some respects, the motor analogue of the globus.

Dr. Norman Kerr said that Sir Andrew Clark, in his lucid, thoughtful, and suggestive paper, had, not for the first time, laid the profession under a deep obligation by directing attention to a diseased condition dependent on a physiological crisis. In their every-day work their energies were too apt to be concentrated on resolute attempts to cope with grave maladies, so that too little consideration was, as a rule, given to minor morbid functional neuroses arising, in great measure, from some disturbance of a normal process. He had seen a number of cases of this convulsive, earthquake cough of puberty, all, except a few unimportant cases of males, having been of the female sex. Most of the cases had been resident in the country. One was that of a girl of 14, who had been considered by several practitioners to be labouring under permanent chest disease, but who, after two years of almost hourly tussile spasmodic seizures, had been cured in about three months by determined treatment of the neurotic conditions, hygienic measures, air, exercise, occupation, and valerianate of zinc with asafœtida, nux vomica, and iron. External application of belladonna to spine and bathing were also insisted on. In this case there was very scanty feeding, from poverty. The recovery had been complete.

Dr. Stephen Mackenzie remarked that, though references to the complaint so admirably described by Sir Andrew Clark were scanty in literature, it would probably be found that most physicians of experience were acquainted with it. In one respect his experience differed from Sir Andrew Clark's, and that was as to the age. He had certainly seen it more often in children of between 10 and 14 years than above the latter age. In order to be able to treat it successfully one must have a clear opinion as to its nature. He regarded the cough as due to the uncontrolled activity of some lower centre, whether in the medulla or elsewhere, owing to the inhibition of some higher centre. He thought it was, as Dr. Gowers

suggested, a kind of "habit-spasm" allied on the one hand to chorea and on the other to hysteria. It probably originated in the first instance in local causes. The cough attracted attention; the patient, if a girl, became the subject of attention and consideration; if a boy, a nuisance. The desire for attention kept up the complaint. His plan of management of such cases was not by drugs but by moral treatment. There were two ways of carrying this out. One was by ignoring it, and this policy of wholesome neglect on the part of the parents was, whilst difficult to carry out, often entirely successful. The other plan was punitive—shower baths or electricity—sufficiently strong to be painful. He did not think it was important to what part the faradic current was applied, though he was in the habit, from custom, of applying it to the throat. The principle was to make it more unpleasant than the effort to restrain the cough. He had never known this plan to fail in children, but he mentioned a case of allied condition in a middle-aged woman in which, though always successful when carried out in the hospital, when the patient returned home she relapsed. He wished to add that in his experience the disease was more common in boys than in girls, and he was in the habit of calling it the "barking cough of boys."

Dr. DE HAVILLAND HALL was unable to see how Sir Andrew Clark differentiated the "barking cough of puberty" from a similar kind of cough met with in neurotic patients, particularly of the female sex; he could not therefore regard the affection as a clinical entity. He mentioned the case of a lady aged 26, who had suffered from a barking cough off and on for five years; after innumerable plans of treatment had been tried she went to Australia in the hope that the sea voyage would cure the cough. On the voyage she had so severe an attack of spasm of the larynx that tracheotomy was obliged to be performed, and the tube was worn for four months. There was no return of the cough after the operation and the patient was free from it when last heard of, about a year later. Sir Morell Mackenzie mentions a similar case to that; though usually more troublesome than serious, these cases are not altogether devoid of danger. In his experience the patients were not as a rule over-fed, and tonic treatment with moral persuasion did good. He had found benefit from the combination of a grain of valerianate of zinc with two grains of pil. asafœtid. co. The application of a cantharides plaster to the spine over the 4th and 5th dorsal vertebræ had also been beneficial.

Dr. FELIX SEMON began by stating that the literary references to the affection so ably dealt with by Sir Andrew Clark were not so exceedingly few as would appear from his remarks. In Gottstein's work on 'Diseases of the Larynx' a much larger collection than that given in the paper might be found, accompanied by very interesting observations of many authors on the pathology of the disease. With regard to the latter it was still a very obscure one and he regretted that the discussion, animated as it had been, had thrown no light upon it. The only points of view inwhich all speakers seemed agreed were that there must be (1) an increased peripheral reflex irritability, (2) an unstable equilibrium in the higher centres themselves. According to his own observations, the cough in question could hardly be called a cough of "puberty," one of his patients having been 9 and another 22 years old. Both had shown the very characteristic symptoms so graphically described by Sir Andrew. In his experience there were two varieties of this rhythmic cough, the one in which it occurred in single attacks, the other in which there was practically no interruption at all during waking. The latter variety was that of

which he had seen more cases. With regard to treatment, he could only say that he had not observed the least good from any topical applications to the larynx, and he much regretted that Sir Andrew had thrown cold water, so to say, upon the plan of a sea voyage. He was not in the least able to explain its particular mode of action, but, in six out of the seven cases in his private practice in which it had been tried, the cough, which had previously resisted all possible forms of treatment at the hands of most experienced members of the profession, had disappeared within a short time—three weeks at the utmost—after the commencement of the voyage, and had not returned. That it was not the sea-air alone which effected such results was conclusively proven by the fact that in some of these cases previously a residence at the seaside had been tried, with in part negative, in part only palliative and temporary, results. Considering finally that according to Sir Andrew's own admission he had not been able to effect more than a relief of the symptoms and no actual abbreviation of the disease itself by the therapeutical measures recommended in his paper, it seemed to him (Dr. Semon), though he did not in the least underrate the questions of expense and of inconveniences of a sea voyage for adolescents, well worth trying, if circumstances permitted, a plan which, as his experience had shown, was likely to effect so remarkable a shortening of what was undoubtedly a very unpleasant and harassing affection.

Dr. Maclagan mentioned the case of a young lady in whom the convulsive cough alternated with convulsive movements of the left arm and double internal strabismus. When she had the cough she did not have the convulsive movements; when she had the convulsive movements the cough disappeared. No cause for the attacks could be found. The late Dr. Matthews Duncan also saw her, with the object of finding out whether or not any uterine or ovarian cause existed. None was found. He suggested shaving and blistering the top of the head. It was decided to do this; but the razor had hardly touched the head when suddenly all the symptoms vanished, and the patient seemed to be quite well. She remained well that day, but on the following day the old symptoms recurred. She had all sorts of tonic treatment; and this went on for nine months without apparent benefit. Then she went to Paris to be under Charcot's care. She remained there for six months, and came back much the same as she was when she left. She went home to the country and led an ordinary life, taking no medicine. In about a year all her symptoms had disappeared, except a slight internal squint of right eye. It was not possible to say what gave rise to the illness; but the alternation of the convulsive cough with convulsive movements of the left arm and muscles of the eye would not indicate any special disturbance of a respiratory centre.

Dr. Percy Kidd would not occupy the time of the meeting by alluding to the convulsive cough of young women familiar to most people, but wished to draw attention to the case of a young boy between 12 and 13 years of age. The patient, who had suffered from severe paroxysms of convulsive cough closely resembling those so lucidly described by Sir Andrew Clark, had been treated in the Brompton Hospital unavailingly for two months, when he became an out-patient under the speaker's care. A curious feature of the case was the fact that a paroxysm of cough could at once be excited by touching the throat externally in the region of the pharynx, larynx, or trachea. Careful examination revealed no disease of the respiratory or other organs with the exception of moderate enlarge-

ment of the tonsils. Sedative remedies, including cocainisation of the pharynx, having proved ineffectual, it was proposed to the boy's mother to remove the tonsils, and with her consent this was done by the resident medical officer. From that time the cough ceased, and when the boy was last seen, some months later, there had been no recurrence. The cough in this case was attributed to a morbidly excitable condition of the nervous system, and especially of the cough centre, combined with peripheral irritation from the throat.

Dr. EWART agreed with the views expressed in the paper and with the remarks made by various speakers. They all converged towards the conclusion that two elements were concerned in the production of the affection, the one constitutional, and the other local. The former consisted in an undue excitability of the nervous system. As to the peripheral factor, there was much pointing to its localisation within the area of the 5th nerve. A similar mode of origin was often observed in the most important of all the respiratory neuroses—asthma. The success of counter-irritation applied to the scalp also supported this view. He was inclined to regard convulsive cough as often dependent upon local irritation set up by the second dentition. In one of his cases this causation appeared to be clearly made out, and the administration of bromide of ammonium afforded relief; and in other cases the same origin was strongly suspected. So trivial and familiar a source of irritation was universally recognised, but some did not attach to it the same importance as did the speaker. The sudden cessation of the cough was in harmony with the culminating event of eruption. Whilst believing in this as a determining cause in many instances, Dr. Ewart did not fail to recognise that the most important etiological moment was the heightened nerve excitability so well described by Sir Andrew Clark.

Sir ANDREW CLARK, in reply, said that the rhythmic, paroxysmal, musical, and regular character of the cough he had described differentiated it *in toto* from the nervous cough, which latter he might describe as characterless. He thought that probably the seat of it was in those spinal ganglia which had recently been shown to exercise such control over the respiratory movements. The cases almost entirely occurred within the period of pubertal influence. Over-feeding occurred in all his cases, but he mentioned it merely as a coincidence.

January 5th and 19th, and February 2nd, 1891.

THE LETTSOMIAN LECTURES: ANÆMIA, ITS PATHOLOGY, SYMPTOMS, AND TREATMENT.

By Stephen Mackenzie, M.D., F.R.C.P.

LECTURE I.

The Composition of the Blood.—The Plasma.—The Formed Elements.—The Red Corpuscles; their Sources, Life-history, and Fate.—The Seats of Blood Destruction.—The Colourless Corpuscles; their Sources, Functions, and Fate.—Blood Platelets.—Elementary Granules.—Hæmoglobin.—The Pathology of Anæmia.—Defective Hæmogenesis or Excessive Hæmolysis, or the two combined.—Anæmia, Secondary and Symptomatic, or Primary and Idiopathic.

Mr. President and Gentlemen,—In choosing anæmia as the subject of these lectures I have been influenced by the following considerations:—

1. Anæmia is a condition which we have to take into consideration almost every day of our lives, and has an interest, therefore, for those practising in every department of medicine. 2. It is a disease which has in recent years received a great deal of careful study at the hands of some of the ablest investigators in our own and other countries; and, 3, because it is a subject to which I have myself devoted considerable attention. It has seemed to me that the time has come when the careful and detailed work which has been accomplished should be collated, and thus allow of our taking a comprehensive view of the whole subject. How imperfectly I have done this I am only too fully conscious, but I still hope that the bringing together the labours of many workers on a subject of great intricacy will not be without some value. I must apologise in the next place for some elementary details with which you are all acquainted, but with which I am obliged to preface the consideration of anæmia, for, unless we define what we agree to consider the normal condition of the blood, we cannot point out what are departures from it.

Should the blood be regarded as an internal medium or as a tissue? The majority of writers and physiologists are inclined to follow Claude Bernard, that the blood is not to be regarded as a fluid tissue, but as an internal medium, bearing the same relation to the constituent tissues as does the external medium to the individual. If we take this view, it must I think, be the plasma or lymph, rather than the blood itself, which should be regarded as an internal medium, seeing that it is the plasma alone which penetrates into the intercellular spaces, bathes the cells with fluid, and thus constitutes what Dr. Harry Campbell has happily and aptly termed "the internal cell environment."* On the other hand, arguments have been advanced in favour of regarding the blood as a tissue, especially recently, by Dr. William Hunter.† Whether regarded as a medium or a tissue, a mechanism exists which in all ordinary circumstances maintains a standard, within certain limits, both as regards its quantity and quality.

With regard to the quantity of blood, it has been shown to vary in relation to the body weight in different animals, and that in man it is about one-thirteenth of the body weight. The quantity of blood is liable to constant variations whilst oscillating round an equilibrium, but unfortunately we have no means by which, clinically, we can estimate the quantity. The mere capacity and fulness of the superficial vessels teach us nothing of the volume of the blood, as we cannot at the same time estimate the amount of blood contained in the capillaries and small veins of the abdominal viscera.

The Composition of the Blood.—The normal blood consists of formed elements floating in a fluid—the plasma.

The Plasma or Liquor Sanguinis.—The plasma is a clear fluid of a faint straw colour, consisting of fibrin, or fibrin factors, and serum. It is alkaline in reaction, owing to the presence of disodic phosphate and sodium bicarbonate. The reaction of the blood has received but little attention, but von Jaksch has shown that it is diminished in pernicious anæmia, leukæmia, and in fever;‡ and Haycraft states that the reaction of the blood in different conditions may vary as widely as that of the urine. The specific

* 'The Causation of Disease,' by Harry Campbell, M.D. Lewis, 1889, p. 18.

† "Transfusion," 'British Medical Journal,' July 20th, 1889.

‡ 'Zeitschr. f. klin. Med.,' xiii, S. 350.

gravity of the plasma varies from 1026 to 1029. The specific gravity of the blood as a whole varies from 1035 to 1068 (Lloyd Jones), or from 1045 to 1075 (Landois). Some valuable observations on the specific gravity of the blood have recently been made by Dr. Lloyd Jones.* According to his observations, it is highest at birth, reaches its minimum between the second week and second year, and rises gradually to a point attained in the male between the ages of 35 and 45 (1058·5), and in the female after the climacteric (1054·5). It tends to be higher in the male than in the female, and is probably only the same in the two sexes at the extremes of life. The effect of pregnancy is to diminish the specific gravity to a slight extent within the limits of health. Solid and liquid food cause a fall in the specific gravity, if no alcohol is taken. Gentle exercise causes a fall, but violent exercise and free perspiration cause a rise. It is subject to "diurnal variation," tending to fall during the day and rise during the night's rest. These observations appear to me important, as the specific gravity of the blood is likely to cause an alteration in the red blood corpuscles. Ehrlich has shown that a change of the red corpuscles from the normal to that of degeneration, as displayed by staining with eosin-hæmatoxylin, can be demonstrated in simple traumatic anæmia a very short time after the bleeding. It thus appears, according to him, that the diluted condition of anæmic serum, as such, exercises an injurious influence upon many blood discs. In pernicious anæmia the specific gravity is usually between 1028 and 1038. With fibrin and its formation we shall not concern ourselves, merely remarking that the amount of fibrin seems to present remarkable variations in anæmic conditions. The serum consists of proteids—serum-albumin and paraglobulin (or serum-globulin)—fats, extractives, salts, water, and a peculiar yellow pigment (choletelin—MacMunn), on which its colour depends. In anæmia, especially in its severe forms—such as hæmorrhage—there is a diminution in the solids and an increase in the water, so that, temporarily at least, a condition of hydræmia exists. The albuminous constituents, of which hæmoglobin is the chief, are greatly diminished. I do not propose to dwell on the chemical composition of the blood, which will be found very fully dealt with by Dr. Sidney Coupland in his Gulstonian Lectures.†

* 'Journal of Physiology,' 1887.

† 'British Medical Journal,' 1881.

The Formed Elements.—The coloured or red blood corpuscles are biconcave discs of yellow colour. Their average size is 7·5μ (micromillimetres) in diameter, but they range between 6·5 and 8·5 to 9μ, with a thickness of 1·9μ. The percentage of less or greater diameter varies in different individuals. In the newly born and very young child the range is very great, from 3·3μ to 10·3μ, and in certain diseased conditions there is a reversion to this embryonic or infantile type. In such cases the range between the smaller and larger forms may be from 2·5μ to 14μ. To these aberrant forms the terms microcytes and megalocytes have been applied (Osler). The red corpuscles consist of a framework or stroma composed of globulin, which is impregnated with a red pigment consisting of hæmoglobin, which is loosely associated with the stroma, from which it may be separated. The stroma appears to have a spongy structure, which soaks up the hæmoglobin. The coloured corpuscles have no distinct membrane. The hæmoglobin may be split up into a proteid belonging to the globulin group, and into a pigment containing iron—hæmatin. The red corpuscles not only vary in size, but in colour, according to the amount of hæmoglobin they contain. This variation in colour, which can scarcely be observed in health, becomes very conspicuous in anæmia. Apart from the quantity of hæmoglobin in a corpuscle, there is no evidence of variations in its quality beyond the amount of oxygen combined with it, and which constitutes the difference between the bright scarlet colour of arterial blood and the purplish colour of venous blood.

Nucleated red corpuscles are found in the embryo, but never, as far as I am aware, in post-embryonal life in conditions of health. As regards the number of red corpuscles in healthy blood, 5,000,000 per cubic millimetre is generally accepted as the average; but, as Hayem,* Laache,† and others have pointed out, the residents in towns may have only 4,600,000 or 4,500,000 per cubic millimetre when in good general health. The number 5,000,000 per cubic millimetre may be exceeded, and, taking the two sexes together, the range of health may be said to extend from 5,500,000 to 4,000,000 per cubic millimetre; or, taking the sexes separately, in men the range is from 5,500,000 to 6,500,000, and in women from 5,000,000 to 4,000,000. In this country, where the estimations are usually

* G. Hayem, 'Du Sang.' Paris: G. Masson, 1889.

† 'Die Anæmia,' von S. Laache, Christiania, 1883.

made by Gowers's very convenient hæmacytometer, the corpuscles are expressed in percentages of the normal—the normal = 100. Expressed centesimally, the range of the two sexes is from 110 to 80; or of the sexes separately, that of men ranges from 110 to 90 per cent., that of women from 100 to 80 per cent.*

The Sources of the Red Blood Corpuscles—Hæmogenesis.—At an early stage of embryonal life, when blood makes its appearance, it is a colourless fluid containing only white corpuscles (each with a nucleus), which are derived from certain cells of the mesoblast. These white corpuscles change into red ones, which become flattened, and their protoplasm gets homogeneous and of a yellow colour. All through embryonal life new white corpuscles are transformed into red ones. In the embryo of man and mammals these red corpuscles retain their nuclei for some time, but ultimately lose them. New nucleated red blood corpuscles, are however, formed by divisions of old red corpuscles.† Such is a concise statement by Dr. Klein of the development of the red corpuscles during embryonal life, and no differences of opinion practically exist on this subject. We see foreshadowed here the two modes of development of the red corpuscles believed by observers to take place in post-embryonal life, that is, from the colourless blood corpuscles and from pre-existing red corpuscles, each of which finds supporters. Whilst many histologists and physiologists believe that the red corpuscles are transformed leucocytes, of late a growing uncertainty has spread whether the evidence upon this point is conclusive. Practically all are agreed that a nucleated red blood corpuscle is the immediate precursor of the fully developed non-nucleated red blood corpuscle. The seats of origin of the red corpuscles are generally believed to be the bone marrow, the spleen, and the lymphatic glands, whilst some believe the thyroid body has blood-producing powers.

The tendency of all recent researches following the teaching of Neumann has been to narrow down the birthplace of the red corpuscles in man to the marrow of the bones, but we shall have to pass in review the evidence in favour of the other asserted sources. That the marrow takes a very active share, or the chief

* The percentage may be expressed in millions per cubic millimetre by multiplying by 50,000; or millions per cubic millimetre as percentage, by dividing by that number.

† Klein's 'Elements of Histology,' p. 14.

share, in blood production is now universally admitted, but the details of the development of the red corpuscles are still much disputed. The evidence that the marrow is concerned in the production of the red corpuscles is mainly that nucleated red corpuscles are always found in this situation, and that their numbers are enormously increased when blood formation is in a state of high activity. Moreover, when hæmogenesis is very active, the yellow marrow of the diaphyses of the long bones and the cancellous tissue of the flat bones disappears, and is replaced by red marrow, having the colour and consistence of raspberry jelly.

The development of the red corpuscles being admitted to take place in the marrow, there are differences of opinion as to the mode in which it occurs. It is believed by many that the red corpuscles arise from the parenchyma of the marrow, either from marrow cells (Neumann, Osler, &c.), or from leucocytes (Kölliker, Fuhrner, Malassez and Gibson), and that their mode of origin is *extravascular*, whilst Bizzozero and Torre, and more recently Denys,"* believe that it takes place within the lumen of the vessels, that is, that it is *intravascular*. Bizzozero has modified his original opinion in the light of his further observations. At one time he believed that the red corpuscles developed out of colourless corpuscles, the latter passing out of the marrow pulp into the veins of the marrow before assuming hæmoglobin (Gibson). Bizzozero and Denys further agree in recognising an early or immature stage of the red corpuscles in certain cells within the vessels which they call "erythroblasts," but they differ in one important particular, in that Denys believes the erythroblasts are transforming leucocytes, whilst Bizzozero maintains that the erythroblasts are young red blood corpuscles containing hæmoglobin.

We owe so much of our knowledge of the structure and functions of the marrow to Bizzozero's investigations, and his latest paper† appeared to me to place the facts so clearly and capable of verification that I have obtained the assistance of Dr. James Galloway to carry out a research on the marrow on Bizzozero's lines. These confirm in all essential particulars the conclusions

* "La Structure de la Moëlle et la Genèse du Sang chez les Oiseaux," 'La Cellule,' tome iv, fas. 1.

† 'Archiv für mikrokospische Anatomie,' Band xxxv, Heft 4, July, 1890, S. 424.

arrived at by Bizzozero, and I have the opportunity, thanks to Dr. Galloway's help, of illustrating the mode of development of the red corpuscles.

In only one particular have we been unable to follow Bizzozero, and that is on the condition of the marrow in animals rendered anæmic by bleeding, and this is owing to the restriction which is placed on experimental research on animals in this country. I have not had the time to obtain the necessary licence by which we are fettered in this country. The observations we have made have been on fowls and pigeons, as Bizzozero and others have found that the marrow of birds is peculiarly favourable for the study of blood development, the ovoid shape and nucleated condition of the red corpuscles rendering confusion between them and other cells more easily avoidable. Indeed, one may go so far as to say that the marrow of birds should be previously studied before one enters upon the investigation of human marrow. Neumann, Malassez, Geelmuyden, and Gibson believe the red corpuscles originate from marrow cells, though the former at one time thought they arose from leucocytes in the bone marrow.

Gibson, whose paper* is a valuable contribution to medicine, from which I have gained much information, has described very fully and carefully the changes that take place in their development from marrow cells. He describes a thin ring containing hæmoglobin at the periphery of the cell; this gradually increases so as to fill the whole space between the cell envelope and the nucleus, and as the hæmoglobin-tinted perinuclear substance increases in amount, the nucleus appears to become smaller and to retreat towards the centre of the cell, the cell as a whole at the same time becoming smaller. He believes that the nucleus is the active agent in attracting the hæmoglobin to the cell, and that when its function is fulfilled—that is, when the cell is fully charged with hæmoglobin—being of no further service, it disappears. He believes also that nucleated red blood corpuscles arise in the marrow from white blood corpuscles, and that the white cells which are transformed into nucleated red cells in the bone marrow are not necessarily all produced there, conjecturing that many of them are produced in the lymphatic glands and

* J. Lockhart Gibson, "On Blood-Forming Organs and Blood Formation," 'Journal of Anatomy and Physiology,' vol. xx, 1886.

carried to the bone marrow, there to enlarge and pass through the various stages of transformation.

Osler* also believes that the nucleated red blood corpuscle is derived from the clear-bordered marrow cell, there being, in his opinion, no essential difference between the protoplasm of the two cells. He further thinks that the colourless marrow cells are derived from small lymphoid elements resembling free nuclei (" primary lymph cells," " protoleucocytes "). The transformation of the nucleated red cell into ordinary red blood corpuscle is brought about by the gradual disappearance of the nucleus. Osler, too, believes in the development of red corpuscles within the large myeloplaques, and in the elongated cells of the stroma of the marrow, in the latter case thus supporting the contention of Schäfer† that red corpuscles may develop within connective tissue corpuscles.

To return to Bizzozero's observations and our own. Bizzozero advises that the fresh marrow should be teased and examined in its natural fluid, and in a simple solution of sodium chloride (0·7 per cent.), and preparations of fresh marrow hardened in various ways should be examined unstained, and stained with various reagents. Each of these methods is necessary as a control to artificial changes which may be occasioned by the modes of treatment. It appears to me particularly necessary that *sections* of marrow, suitably hardened, should be always examined, as in teased preparations it is difficult or impossible to determine whence cells which are taken to be developing blood corpuscles come, whether from within the blood vessels or from the parenchyma of the marrow. When a section of marrow (fowl's) is examined with a low power, it is noticed that the vascular network in the long bones consists of a few arteries running in the long axis; from these a few small arteries are given off at an acute angle, which course thence towards the periphery of the marrow, whence they lose themselves in a few narrow capillary vessels. These arterial capillaries open into a close network of very wide venous capillaries, which, anastomosing with one another, wander through the whole marrow in a centripetal direction, and empty themselves into large veins which run parallel

* ' British Medical Journal,' 1886, p. 862.

† Creighton has described the development of red corpuscles in blood cysts by a process of budding from embryonic cells lining their walls.

with the arteries in the long axis of the marrow. The arrangement may be compared to a wheel, the arterial and venous trunks corresponding with the axis and the venous capillaries with the spokes. The great disproportion between the calibre of the arterioles and venules, which is very striking, occasions as a necessity that the blood stream must be peculiarly slow, and, therefore, favourable to changes taking place within the venules. Moreover, when anæmia is produced, the fat cells are absorbed, allowing of an increase in the parenchyma (lymphatic tissue) and dilatation of the venules. Bizzozero and Denys assert that these capillaries have distinct walls, their outlines being mapped out by a regular border, interrupted here and there by oval elongated nuclei. We can confirm this statement, which is an important one. Some observers believe these venous capillaries have no walls, but are simply tunnelled out of the parenchyma, so that no boundary exists between the parenchyma and the venules.

In extreme degrees of anæmia the venules become so much dilated that they are in many places actually in contact with one another. With sections of marrow hardened in Müller's fluid or in corrosive sublimate, and stained with picric acid, alcohol, and hæmatoxylin, it can be ascertained that there are no hæmoglobin-containing cells external to the blood vessels. Within the arteries and venous trunks blood corpuscles of the usual type found in the blood elsewhere are seen, but in the venous capillaries there are found young embryonal forms of red corpuscles undergoing division. Even in the marrow of healthy animals one observes a tendency of the corpuscles in the venules to arrange themselves in the following manner:—In the centre of the lumen are red corpuscles fully developed and deeply stained with hæmoglobin. In the peripheral part of the vessel are found near to, or in contact with, the wall of the vessel a varying number of leucocytes, and in the intermediate area, or middle zone, are a number of cells undergoing mitosis, and which are composed of a spherical nucleus, which is surrounded by an envelope of hæmoglobin-containing protoplasm. These are the young red corpuscles undergoing indirect division, to which, following Löwitt, the name of "erythroblasts" has been applied by Denys and Bizzozero. In animals made highly anæmic by bleeding, Bizzozero describes (1) the venous capillaries are markedly widened at the cost of the parenchyma; (2) in the parenchyma is seen an atrophy of the fat cells and a hyperplasia

of the leucocytes; (3) the lumen of the venous capillaries is filled out in a most marked manner by young red blood cells and leucocytes; the fully grown red blood cells form narrow streaks n the axis of the vessels; the leucocytes, as well as the young red blood cells (the latter in greatly the preponderating quantity), increase in number actively by the process of mitosis. So far Denys and Bizzozero travel together, but regarding the nature of the erythroblasts they differ. Denys maintains that the erythroblasts are developmental forms free from hæmoglobin between the leucocytes on the one hand and the fully developed red blood corpuscles on the other hand. Bizzozero, on the contrary, asserts that the erythroblasts always contain hæmoglobin, and that they arise from previously existing hæmoglobin-containing cells by a process of karyokinesis. He points out that Denys's observation that the erythroblasts contain no hæmoglobin has been caused by the influence of corrosive sublimate upon the hæmoglobin, and that by examining fresh marrow, marrow hardened in Müller's fluid, and by processes of staining recommended by H. F. Müller and Foà, one may satisfy himself that the youngest forms of red cells already contain a trace of hæmoglobin. Many of these are dividing by mitosis, and he asks, "Can there well be a younger element than a cell which is not yet independent, as it still forms part of another cell from which it takes origin?" Dr. Galloway's and my own observations entirely confirm those of Bizzozero that the erythroblasts at all stages contain a greater or less amount of hæmoglobin.

Bizzozero's conclusions may be briefly summarised by the following statement:—That in the marrow of birds (1), owing to the great disproportion between the calibre of the arteries and venous capillaries, the blood must circulate in the latter with extraordinary slowness; (2) that in the venous capillaries the blood is different to what is observed in the vessels of any other part of the body, in that the fully developed red blood cells occupy only the axis of the lumen; around them are situated quite up to the vessel wall two sorts of elements; leucocytes are to be found in larger quantities than is usually the case in other parts of the body, and red blood cells which are to be recognised as young red blood cells (erythroblasts) by their round or slightly oval shape, the characters of their protoplasm and their nucleus Moreover, a certain number of erythroblasts are seen in a state of

indirect division. The vessels of the marrow are the only regions of the organism of birds, in their adult condition, in which erythroblasts and their developmental forms are found; in these vessels alone is one able to locate the source (herd) of the red blood cells. (3) In anæmia the venous capillaries are enormously dilated at the expense of the parenchyma, especially the fat cells, the lymphoid tissue increasing, and the leucocytes showing more active mitosis. In the lumen of the vessels the number of leucocytes increases as well as the erythroblasts, which almost completely fill up the lumen of the venous capillaries; the fully grown red blood cells occupy only a small space corresponding to the axis of the vessel. The number of leucocytes which are found in a state of mitosis is considerable, but that of the erythroblasts is great. (4) On account of the characteristics of its structure, the network of the venous capillaries of the marrow is distinguished from all other networks of the body. It subserves the production of two morphological elements of the blood, namely, the leucocytes and the red blood cells. In the manufacture of the first are associated with it many other organs of the body, and among them also the parenchyma of the marrow itself; in the production of the latter, on the other hand, the marrow is alone concerned. (5) The vascular network of the marrow may, therefore, be considered as a true endovascular organ—a gland which secretes morphological elements. Bizzozero and Torre,* in 1884, showed that the blood development in the marrow is the same in all Vertebrata as in birds, but the former states he has not found blood corpuscle-containing cells in the marrow of birds. The three main points appear to me to be quite certain, at least as regards the marrow of birds: (1) that development of red corpuscles takes place entirely within the vessels (venules); (2) that the young red corpuscles arise from pre-existing red corpuscles; (3) that the young red corpuscles are formed by a process of indirect division (karyokinesis).

Thus, whilst all observers are agreed that the nucleated red blood corpuscle is the immediate precursor of the fully-developed non-nucleated red corpuscles, they are divided into three camps as regards the origin of the nucleated red blood corpuscles: (*a*) Those who believe that they arise from marrow cells; (*b*) those who believe in their derivation from colourless corpuscles (leucocytes,

* 'Virchow's Archiv,' Bd. xcv.

leucoblasts, erythroblasts)—they have not clearly indicated which of the several varieties of the colourless corpuscles is the parent of the red corpuscles; (*c*) those who deny altogether the development of red corpuscles from white corpuscles, and who believe that all red corpuscles arise from pre-existing red corpuscles by a process of indirect division (karyokinesis). Further, those who adopt the two first views believe in the extravascular origin of the red corpuscles; whilst those adopting the last believe it to be entirely endovascular. It is still a matter of uncertainty as to how the nucleus disappears. Some believe, with Rindfleisch and Ehrlich, that it passes bodily out of the cell; others, with Kölliker and Neumann, that it breaks down and becomes absorbed in the general cell substance. No entirely satisfactory explanation has been hitherto afforded of how the nucleated red corpuscle derived from a marrow cell or leucocyte acquires its biconcave form. That this very special characteristic of the fully-grown corpuscle is no accident we may feel sure, but evidence of a developmental tendency. If we accept the view that the nucleated red corpuscle is derived from a pre-existing red corpuscle no difficulty is experienced, as we have simply an instance of a cell reaching its ultimate and full development, just as a ciliated columnar cell is evolved out of a flat or spherical embryonic epithelial cell. On the other hand, great difficulties are presented in regarding the fully-developed red corpuscle reaching such a highly-specialised form from cells offering such wide differences as the colourless corpuscles and marrow cells.

Blood-forming Function of the Spleen.—In the human subject the spleen appears to have a limited capacity of blood generation in extra-uterine life. Neumann and Freyer have been led to the conclusion that the spleen is practically not a blood-forming organ. Bizzozero and Salvioli have demonstrated that it has decided, if not very active, blood-forming powers. Much of the difference of opinion that exists has arisen from the kind of animal that has been the subject of experiment, for the relative share taken by the cytogenic organs in blood formation differs considerably in different classes. Whilst caution must be exercised in inferences drawn as to the hæmopoietic functions of the human spleen from observations and experiments on animals, seeing how widely they differ amongst themselves, as far as the evidence afforded by the effects of removal of the spleen goes, it is in favour of the human

spleen behaving in this respect like the spleen of the dog. A case of removal of the spleen, recorded by Credé,* was followed by decrease in the red corpuscles—many of those present were of small size, and some nucleated—and by an increase, relative and actual, in the number of white corpuscles. The blood gradually changed to the normal again by the end of four months and a half. Bizzozero and Salvioli found in guinea-pigs and dogs, made anæmic by bleeding, nucleated red corpuscles in greater or less number. In dogs the spleen became "rose-coloured and very much swollen," and, by simply looking at it, they could say "there will be nucleated red cells here" (Gibson). Dr. Gibson† has recently made some careful and important investigations on the hæmopoietic properties of the spleen, and concludes that the blood-forming action of the spleen is, in extra-uterine life, a subordinate one, but that when the reserve blood-forming capabilities of an animal are called on its activity is increased. That the spleen gives rise to colourless corpuscles is generally admitted. They arise, partly by indirect division from other colourless corpuscles, and partly by budding from the cells of the stroma (Klein).‡

Blood-forming Functions of the Lymphatic Glands.—It has long been held that lymphatic tissue, whether arranged as glands or occurring in patches of adenoid connective tissue (as in the agminated and solitary glands of the intestine), are the chief producers of colourless corpuscles. It is also thought they may have the power of transforming colourless into red corpuscles. That this power, if it exists, is very sparingly exercised in general is evidenced by the well-known characters of lymph. It is, however, possible that when lymph is delayed in its channels it may bring about the conversion of leucocytes into red corpuscles. This has, it is believed by Gibson, been proved, where, after the ligature of the thoracic duct (in a dog), a number of nucleated red corpuscles were found in the abdominal glands. The same results are believed by some to be brought about by the experiments of disease. In cases of chyluria, lymph scrotum, and elephantiasis, in all of which lymphatic trunks are obstructed, the fluid contained in the lymph vessels is said to be reddish, or even as red as blood,

* 'Langenbeck's Archiv,' Bd. xxviii, p. 401.

† *Op. cit.*, p. 473.

‡ 'Quart. Journ. of Microscop. Science,' vol. xv, p. 369.

when unmixed with chyle. Dr. Gibson thinks that the normally scarcely existing powers of blood transformation in health may be called into greater activity by demands upon the blood-forming powers of the individual. Foà and Salvioli* found red corpuscles forming endogenously within large protoplasmic cells in lymphatic glands.

Blood-forming Properties of the Thyroid Body.—It has recently been suggested that the thyroid body, if not in ordinary circumstances a blood-producing organ, make take on this function when all the latent capacity of blood-formation of the organism is called into play. The evidence on which this opinion is based has been shown by Gibson to be very meagre and inexact, and it may be accepted that the thyroid body has no hæmopoietic functions.

We have thus considered all the usually ascribed sources of blood formation, and find that it practically amounts to this: That in post-embryonal life the red blood corpuscles have their birthplace in the marrow, though a slight power of hæmogenesis may exist in the spleen, and possibly in lymphatic structures. We now turn to the other end of the life of the red blood corpuscles.

The Seats of Blood Destruction: Hæmolysis.—The life of every cell is limited, and the red blood cell is no exception to this universal law.

We have abundant indirect evidence of destruction of red corpuscles in the formation of bile, the pigment of urine, and other pigments derived from hæmoglobin. The average life of a red corpuscle has been estimated by Quincke at about two weeks. The most active destruction appears to take place in the portal system of blood vessels. In the spleen, marrow of bones, lymphatic glands (and in the liver of the embryo), are found large cells containing blood corpuscles. The corpuscle-containing cells may contain one or two, or even as many as ten or twenty, red corpuscles in their interior, in all stages of disintegration, from the almost normal red corpuscle to mere granules of blood pigment. From the occurrence of these blood corpuscle-holding cells in the organs of blood formation, one conjectures that by breaking down effete corpuscles they subserve the purpose of supplying the new corpuscles with the necessary raw material for the building up of hæmoglobin-containing protoplasm.

* Landois and Stirling, 'Text-book of Physiology,' p. 10.

Osler has also described* "included corpuscles" in the blood stream, that is, colourless corpuscles containing one or more red blood corpuscles. They are very rare, and must be regarded as red corpuscles which have come to a premature end, and are being removed by predatory or proctorial phagocytes. In ordinary circumstances, when the red corpuscles are about to disintegrate, according to Quincke, they are taken up by the white corpuscles in the hepatic capillaries, by the cells of the spleen, and the bone marrow, and are stored up chiefly in the capillaries of the liver, in the spleen, and in the marrow of bone. They are transformed partly into coloured and partly into colourless proteids, which contain iron, and are deposited in a granular form or are dissolved. Part of the products of decomposition are used for the formation of new blood corpuscles in the marrow and spleen, and also perhaps in the liver, while a portion of the iron is excreted by the liver in the bile (Landois and Stirling). In pernicious anæmia the liver has been shown by Stahel, Quincke, Peters, Rosenstein, Mott, and others, to contain an excess of iron. It was at one time thought possible that this excess of iron was due to its administration; but the observations of Kobert, Cohn, and Gläveck have proved such not to be the case. Dr. William Hunter has worked out very completely and carefully the difficult problem of blood destruction.† He describes it as of two kinds: (*a.*) *Passive destruction*, which is the ultimate destiny of the red corpuscles which live their allotted span and undergo a process of natural decay. This form is evidenced by the presence of pigment in certain organs of the body, chiefly in the spleen, capillaries of liver, and bone marrow. "The chief character of this pigment is the large and varying size of the individual pigment particles, and their irregular shape." It is never found within the liver cells, but always lies enclosed in leucocytes within the capillaries. (*b.*) *Active destruction*, in which the hæmoglobin, instead of remaining within the red corpuscles, escapes from them into the plasma, and is in great part excreted by the liver as bile pigment. "The chief character of this pigment is the small uniform size and spherical shape of the individual particles. The assumption of this form by the pigment is independent of the action of the cells,

* *Loc. cit.*, p. 862.

† "Summary of Three Lectures on Transfusion," 'British Medical Journal,' 1889, vol. ii.

but the conversion of the hæmoglobin into blood pigment is as much the result of cellular activity as in passive destruction." The pigment is formed from free hæmoglobin, and is found chiefly within the liver cells in the portal zone. In conditions of health no trace of blood pigment is usually found as the result of the breaking up in, and the excretion of this free hæmoglobin from, the body. But when, as in pernicious anæmia, excessive activity of blood destruction takes place, it is found abundantly in the liver cells. The amount of bile pigment formed is an index of the amount of active destruction. "It is greatest during digestion, and is most marked of all five or six hours after the ingestion of food. Since we cannot conceive," Dr. Hunter writes, "of an increased formation of bile pigment without a corresponding greater supply of hæmoglobin in the liver cells, we must conclude that the ingestion of food is followed by increased active destruction." It appears to me doubtful whether we need go quite so far as this latter statement with Dr. Hunter. As far as I can see, there is some force in Voit's remark: "There is no reason why there should be a greater destruction of red corpuscles during digestion than during the fasting state." One can understand that bile pigment formed during fasting must be derived from destruction of red corpuscles, as there is no other source for the hæmoglobin; but in a diet containing flesh, when bile pigments are greatest, the hæmoglobin taken as food and converted into hæmatin is, at least, a possible source of bile pigment, without ever becoming organised into blood. According to Dr. Hunter, with whose conclusions on this point most physiologists are in agreement, the spleen and the gastro-intestinal mucosa are the most active seats of blood destruction.* The effete and many vigorous red blood corpuscles undergo destruction there; their liberated hæmoglobin is carried by the portal blood to the liver, where it becomes converted into bile pigments. The bone marrow is (outside the portal circuit) the only other seat of blood destruction. The share of the spleen (or, at least, the importance of its share) in hæmolysis is not absolutely established. Schäfer has examined the blood from the splenic vein in a large number of animals, and the result of his observations is

* Klein has shown there are reasons for believing that the blood corpuscles are disintegrated by a destructive influence of the matrix of the spleen upon the corpuscles, and not in the manner usually described, the corpuscles being taken up by lymph cells. *Op. cit.*, p. 371.

decidedly against the view that the spleen is an organ concerned in the destruction of red corpuscles.* Dr. Mott has shown that the spleen often does not give the iron reaction, and states that chemical analysis may show no excess of iron, although the liver may contain a great excess over the normal. Dr. Hunter, however, only regards the spleen as *one* of the seats of blood destruction, which goes on when the spleen is removed. Moreover, the presence of iron albuminates in the spleen and bone marrow may be explained by the peculiarities of the circulation in these structures, by the delay in the capillary veins caused by the disproportion between their calibre and that of the arteries discharging into them.†

The Colourless or White Blood Corpuscles (Leucocytes).—Amongst the important advances in our recent knowledge of the blood is the recognition that the colourless corpuscles differ much amongst themselves, certainly as regards their morphological characters, and probably as regards their physiological functions. Speaking generally, the colourless corpuscles are larger than the red corpuscles, having an average size of 10μ, but varying between 4 and 14μ. The colourless corpuscle may be described as a globular mass of protoplasm containing one or several nuclei. They number in health about 6,000 per cubic millimetre (Hayem), but their numbers vary much at different times in the same individual in health, and still more so in disease. Max Schultze, Bizzozero, Heyl, and Ehrlich, and others have pointed out important differences displayed by them. Ehrlich, especially, has started an important inquiry, which promises practical aids to our knowledge. He has shown,‡ not only that the granules he has described in them differ in their colouring by basic, acid, and neutral dyes, but also in their behaviour in solvent fluids, in their size and shape, in the influence upon them of high temperatures (100° to 180° C.), and in their distribution in the body of the cell. Space prevents my doing more than giving the results of his investigations which have some bearing on my present subject:—"(1.) In all acute leucocytoses only the mono- and poly-nuclear forms are increased, while the eosinophile cells are apparently correspondingly diminished. (2.) An increase of eosinophile cells always

* 'Proc. of Physiolog. Soc.,' May, 1890, quoted by Mott.

† 'Practitioner,' August, 1890, p. 81.

‡ 'Zeitschr. für klin. Med.,' 1880, p. 553.

indicates chronic changes in the blood-forming organs. (3.) Decrease in the number of leucocytes and preponderance of the mononuclear forms, when associated, are a sure sign of defective nourishment of the system of long duration. (4.) Severe traumatic anæmia always gives rise to poikilocytosis and the appearance of nucleated red blood corpuscles; frequently also to an increase of polynucleated, eventually of mononucleated, elements. (5.) In leukæmic blood the absolute number of eosinophile cells* is always increased, often to a very great extent." Of the origin of the white corpuscles, all are agreed that they arise from lymphatic structures, the lymph glands, and disseminated lymphatic tissue, for which Dr. Pye-Smith has proposed the comprehensive and convenient name "lympharia," from the spleen and the bone marrow. Apart from their disputed share in the genesis of the red corpuscles, the active part they play in inflammation as established by Cohnheim, they have recently been shown by Metchnikoff, Ruffer, and others to act as "phagocytes" or scavengers, whose function it is to remove from the blood and tissues micro-organisms and other deleterious matters. When their life cycle is completed, or under the influence of too powerful bacterial action, they undergo disintegration, forming the "elementary granules" found in the blood.

Blood Platelets.—Besides the red and colourless corpuscles, the blood contains some smaller formed elements concerning the origin, nature, and functions of which there are as many theories as the bodies themselves have received names. There are the "hæmatoblasts" of Hayem, the "globulins" of Pouchet, the "Körnchenbildungen" of Max Schultze, the "Elementarkörperchen" of Zimmerman, and the "Blutplättchen" of Bizzozero. They are circular or oval in shape, with well-defined margin, and consist of brightly refractile protoplasmic material of a light grey colour, and in the human subject are destitute of nuclei. They vary in size from 1μ to $1{\cdot}3\mu$. Their numbers have been variously estimated. Hayem states that on the average there are 250,000 hæmatoblasts in the cubic millimetre. Osler has calculated them at from 125,000 to 300,000 per cubic millimetre, Fusari at from

* The "eosinophile cell," whose normal and exclusive place of origin is the bone marrow, is a constant constituent of normal blood; frequently there are from 2 to 4 per cent., and their number may be as high as 10 per cent. 'Charité-Ann.,' xiii, 1888, p. 306.

18,000 to 250,000. Hayem and Pouchet regard them as embryonic red corpuscles, and believe they have been able to trace transitional forms between them and the fully developed red corpuscles. Neumann, Max Schultze, Riess, and Ehrlich, on the other hand, believe that they are the result of blood disintegration, though Osler says there is no corroborative evidence of this. Hayem has described them as tinted with hæmoglobin. Osler, on the other hand, denies that the largest platelets contain even a trace of colour, and Bizzozero also says that they contain no hæmoglobin. What appears at first sight to be an argument in favour of Hayem's view is the fact, generally admitted, that when blood regeneration is most active (as, for example, after hæmorrhage) they are most numerous in the blood. On the other hand, Neumann, Ehrlich, Riess, and Leube believe that in such circumstances the chemical changes and thinning of the blood exercise a deleterious influence on the red corpuscles, causing them to disintegrate, and thus give rise to the hæmatoblasts.

There have not been wanting observers who believe that the platelets are *arte facta*—the product of changes in the red corpuscles after they are shed. There is no doubt that similar bodies may be artificially produced, but there seems no real ground for this view of their ordinary production. They are present at the moment blood is withdrawn, and, examined under the most favourable circumstances such as instantaneously, fix the formed elements. Moreover, Bizzozero, Osler, Karl Laker have observed them flowing in the blood streams of living animals, after taking all precautions that the conditions did not interfere with the vascular contents. Their presence, therefore, whatever their significance, must be regarded as established, and of natural occurrence. Hayem believes they arise from lymph corpuscles while yet in lymph vessels; Pouchet, that they arise in the blood plasma itself by a process analogous to the precipitation of fibrin. Schmidt and others believe they are due to disintegrating colourless corpuscles, whilst Bizzozero is unable to decide whence they come. Recently, Gibson, who has examined into the question with great care and impartiality, believes that under the various names given to these formed elements are included two distinct elements, and that confusion between these accounts for much of the discrepancy in the statements of various observers. He prefers the term "microcyte" for them.

I am afraid it is impossible to choose a term free from all objections, but, burdened as this subject is with multitudinous names for the same thing, it appears to me that we should try and agree upon one term. So high an authority on physiology as Dr. Michael Foster has adopted the term "platelet," and it would be a matter of general convenience if all British writers would follow his example. Substituting, then, the term platelet for microcyte, Dr. Gibson's views are put shortly as follows :*—There are (1) coloured platelets which, he believes, are simply fragments of broken-down red corpuscles,† and (2) colourless platelets which correspond to the "Blutplättchen" of Bizzozero, and these, he believes, are mainly the escaped nuclei of the red corpuscles, though occasionally they may be derived from the nuclei of white corpuscles when these disintegrate. It will thus be seen that Hayem and Pouchet are the chief exponents of the doctrine that the platelets are developmental forms of red corpuscles. Whilst there are widely different views as to the nature and origin of the platelets, there is practically universal accord that they are active agents in the process of coagulation, and in the formation of coagula.

The Invisible Corpuscles of Norris.—These have been proved by the researches of Mrs. Ernest Hart‡ and Gibson to be red blood corpuscles decolorised by the method of preparation.

Besides these definite forms, "elementary granules" have been described by Zimmerman and Beale. They are irregular bodies, much smaller than the ordinary corpuscles, and appear to consist of masses of protoplasm detached from the surface of leucocytes, or derived from the disintegration of these corpuscles or of the blood platelets. Others, again, are completely spherical granules, either consisting of some proteid substance or fatty in their nature. The protoplasmic and proteid granules disappear on the addition of acetic acid, while the fatty granules (which are most

* *Op. cit.*, p. 107.

† Osler, too, describes microcytes arising from the breaking down of red corpuscles, and, like Gibson, has watched their artificial production in the shed blood. The former states they are abundant in all forms of anæmia, chlorosis, splenic anæmia, leukæmia, pernicious anæmia, Hodgkin's disease, and in the anæmia of cancer, phthisis, and other chronic affections. They are most numerous in the primary anæmias, especially progressive pernicious anæmia. He adds they are certainly not always produced by post-mortem changes.

‡ 'London Medical Record,' 1882.

numerous after a diet rich in fats) dissolve in ether (Landois and Stirling).

Hæmoglobin.—The functional activity of the blood depends upon the amount of hæmoglobin contained in the red corpuscles. The hæmoglobin is estimated by various methods—in this country chiefly by means of Gowers's hæmoglobinometer, which is extremely easy in employment, and gives sufficiently exact results for clinical purposes. Under conditions of health the red corpuscles contain a fairly uniform amount of hæmoglobin, so that by estimating the number of corpuscles we have a tolerably accurate indication of the hæmoglobin richness of the blood; or, *vice versâ*, by estimating the hæmoglobin we may gauge the number of corpuscles. In anæmia, however, great variations may occur in the relative proportion of hæmoglobin and corpuscles, so that it is necessary to ascertain the functioning value of the blood to ascertain both the corpuscular richness and the hæmoglobin richness. Hayem recommends that the hæmoglobin richness should be expressed in terms of sound blood, which represent the functional value of the blood. If we divide the hæmoglobin richness expressed in healthy corpuscles by the actual number of red corpuscles, we obtain the value of the individual corpuscles. In dealing with percentages all we have to do is to make the percentage of hæmoglobin the numerator and the percentage of corpuscles the denominator of a fraction. Osler has well summarised the variations which take place in the hæmoglobin contents of the red corpuscles:—

1. In chlorosis the hæmoglobin is reduced out of proportion to the reduction of the corpuscles, so that the individual worth of each red corpuscle in colouring matter may be very greatly lowered. Thus, the true anæmia (or functioning properties of the blood) may be very much greater than the number of red corpuscles per cubic millimetre would indicate.

2. In ordinary anæmia from hæmorrhage or from organic disease the average worth in hæmoglobin of each corpuscle usually remains unaltered, and the percentage of colouring matter corresponds with the percentage of the corpuscles.

3. In certain cases of pernicious anæmia the interesting fact has been ascertained that the percentage of hæmoglobin is increased, and the anæmia is in reality not so great as the reduction in the number of corpuscles would appear to indicate.

Karl Laker* has pointed out that the percentage of hæmoglobin (as tested by Fleischl's hæmometer) may be used as a diagnostic point between benign and malignant tumours. In the former the hæmoglobin is not diminished, whilst it is in the latter. My own observations show that in some cases of cancer (where there was no hæmorrhage or discharge) the hæmoglobin reduction was greater than that of the corpuscles.

Having now—I am afraid at wearisome length—considered the blood in health, we are in a position to discuss the departures from this standard, and so far as they relate to anæmia. Anæmia may be defined as a diminution in the quantity of the blood, of its corpuscles, or of its colouring matter. To the first the term *oligæmia* is applied, to the second *oligocythæmia*, and to the last *oligochromæmia*. Though these do not by any means necessarily correspond in all forms of anæmia, yet as a rule they are coincidently modified. They all have this in common, that they indicate "the poverty of the blood in normal functional red corpuscles" (Cohnheim), which is the criterion of the functional activity of the blood.

We have seen that blood corpuscles are constantly being used up in the production of bile, urinary colouring matters, and other pigments derived from hæmoglobin, and the other constituents of the blood are consumed in the processes of tissue metamorphosis and secretions, and that these losses are met by a continual process of blood regeneration, so that a balance existing between the income and expenditure of the blood, a normal though varying condition or equilibrium, is maintained. Anæmia, on the other hand, implies a want of adjustment between the gains and losses. To which end of the process are we to look for the explanation of anæmia? Is it due to defective hæmogenesis or to excessive hæmolysis? Until quite recently, this question, which was raised by every writer on the subject, was incapable of receiving a definite reply. So little was known of the details of blood formation and destruction that it was impossible to say what share modifications of each of these processes took in the bringing about of anæmia. Great advances have been made in the last few years in our knowledge of these processes, and, though there are *lacunæ* to be filled up, we are reaching a position in which we can frame our pathology of anæmia on a logical basis. It is on this account

* Centralblatt f. d. med. Wissenschaft.,' 1887, p. 405.

that I have gone into such detail in what may be regarded as elementary matters, for so long as it was uncertain whether the histological changes observed in the marrow, the spleen, liver, &c., were to be regarded as the causes or the consequences of anæmia, no rational conclusion could be drawn as to its essential nature. We now have reached tolerable agreement as to seats and evidences of blood formation and blood destruction, and are now able to make these the basis of our pathology. We must, therefore, next proceed to discuss the share each of these processes takes in anæmia. It is obvious that, in simple hæmorrhage of all kinds, anæmia begins from excessive loss or expenditure. It is nearly equally certain that, in profuse discharges of pus or serum from the body, there must be a using up of blood, potential or actual, in the processes which bring these about. The blood-forming capacity of the individual may be in full integrity, but the drain may be such that it cannot keep pace with the demand, and anæmia is the result. Besides these obvious causes of blood expenditure, we have recently, from the closer study of certain forms of anæmia, been taught to recognise that an active destruction of red corpuscles may take place, not from an excess of physiological activity in the employment of the elements so destroyed, but by a process entirely pathological. On the other hand, without any undue calls upon it, blood formation may be defective, either in the quantity or quality of the red corpuscles, when again anæmia results. And, lastly, excessive destruction may be associated with defective formation, and thus each may exaggerate the effect of the other.

Some cases of anæmia are clearly connected with some definite morbid process which has primarily nothing to do with blood formation or destruction, but which stands in relation to the anæmia as cause and effect. It is a matter of practical and clinical convenience, as well as pathological propriety, to separate these cases in which no such causes can be ascertained with certainty; though I believe that the ultimate pathological standpoint in anæmia of all kinds must turn on the question of defective hæmogenesis or excessive hæmolysis. Yet, if we do not adopt some such primary division, we shall have to group together conditions clinically widely different. How are we to estimate the activity of blood formation and destruction? By clinical observations and the revelations of morbid anatomy. Clinically, the activity or integrity

of blood formation in anæmia is indicated, positively, by the character of the blood corpuscles, collectively and individually; and, negatively, by the absence of evidence of blood destruction afforded by secretion of faintly-coloured bile and pale urine. Excessive destruction, on the other hand, is evidenced by the dark colour of the urine and fæces produced by hæmolysis, and perhaps by the staining of the skin and other tissues by products of blood destruction. Morbid anatomy teaches us the actual condition of the blood-forming organs, and supplies us with evidence of whether blood destruction has or has not been excessively active in the organs where this is carried on. With the exception of the changes in the blood-forming and destroying organs, the remaining pathological alterations found in anæmia, the fatty changes in the heart and blood vessels, the hæmorrhages, &c., are the direct consequences of the anæmia, dependent upon its degree irrespective of its cause.

Defective blood formation may depend upon deficiencies in the quantity or quality of the food, in its want of variety, or imperfect preparation; upon failure of the powers of mastication, digestion, or assimilation; upon some defect, inherited or acquired, of the hæmopoietic viscera; or upon such unfavourable conditions as deficiency of light and fresh air, unsuitable temperature, and depressing and unhygienic surroundings. Excessive blood destruction depends upon an increase beyond the normal of those processes which cause active blood destruction, as by the processes of fever and inflammation, or the presence of deleterious substances in the blood. In many instances it is difficult to estimate the share which defective hæmogenesis and excessive hæmolysis play.

Anæmia thus depending on such a variety of causes, it becomes necessary to arrange the cases in some order, and the classification which naturally suggests itself, and which has been adopted by most writers on the subject, is into

1. Secondary and symptomatic anæmia; and
2. Primary and idiopathic anæmia.

In secondary and symptomatic anæmia we have conditions in which the blood deficiency is not the essential feature of the disease, and in which the other symptoms present are not explained by the existence of the anæmia. In primary and idiopathic anæmia, on the other hand, the anæmia constitutes the main

feature of the disease, and the other symptoms present are dependent upon it. Dr. Hunter has,* in my opinion, very clearly indicated the criteria of an idiopathic anæmia, which he states should comply with the following conditions: 1. That the changes in the blood (as determined by actual examination) must not only apparently, but actually, constitute the most marked feature of the disease. 2. That the other chief symptoms must, in great part at least, be referable to the altered condition of the blood. 3. With regard to the post-mortem appearances, that the changes in the blood, whether alone or in the blood-forming or blood-destroying organs, constitute the characteristic morbid feature of the disease. Dr. Pye-Smith† would make a third division of cases of anæmia, "formed of all cases of anæmia associated with disease of the cytogenic organs, whether or not leukæmic." These, which undoubtedly form a natural and compact group, clinically and pathologically, should, I think, rather be included under idiopathic anæmia. In leucocythæmia we have not only an increase in the number of colourless corpuscles, but a great deficiency in the red corpuscles, and many of its symptoms are directly due to the anæmia. How is the latter produced in leucocythæmia? Is it due to excessive hæmolysis or to defective hæmogenesis? There is no strong evidence of the former, whilst the marked alterations in one or several of the blood-forming organs are suggestive of the latter. As long as the question of the origin of the red corpuscles from the colourless is unsettled, it is difficult or impossible to give a decisive answer to the question of how the defective hæmogenesis operates. If, as many believe, the red are transformed colourless corpuscles, an arrest of development is a ready explanation. If, on the other hand, all red corpuscles arise from pre-existing red corpuscles, this ready and inviting hypothesis is no longer tenable. I would merely draw attention to one point which appears to me to militate against the view of the deficiency of red corpuscles being caused by non-transformation of colourless into red corpuscles. This is the condition of the marrow in this disease. When red blood formation is active the venous capillaries become dilated at the expense of the lymphoid tissue, and the marrow becomes red. When, on the other hand, there is lymphoid hyperplasia of the marrow, this is at the expense of the

* "Is Pernicious Anæmia a Special Disease?" 'Practitioner,' August, 1888.

† 'Guy's Hospital Reports,' vol. xxvi.

venules, wherein the red corpuscles are produced; the consequence is there is defective formation of red corpuscles, whilst the eosinophile colourless corpuscles, which we have seen are formed in the lymphatic tissue of the marrow, are produced in excessive numbers. Erhlich has, as I have pointed out, shown that the eosinophile colourless corpuscles are always increased in leucocythæmia. In "anæmia splenica" a similar difficulty arises in explaining the deficiency of red corpuscles. It has been attributed, with a good deal of plausibility, to the retention and accumulation in the spleen of large numbers of colourless corpuscles, which should normally be transformed into red corpuscles. This explanation assumes, what has not been proved, that the red corpuscles are transformed colourless corpuscles. Further, we have seen that the spleen has little share in the genesis of red corpuscles. If, moreover, as assumed, the colourless corpuscles are retained in the spleen, as long as the red corpuscle-forming process can be carried on, the lymphatic glands and marrow will furnish abundant colourless corpuscles for the manufacture of red corpuscles, if these really arise from them, as in cases of extirpation of the spleen. The same remarks apply to "anæmia lymphatica" (pseudoleukæmia —Hodgkin's disease). The lymphatic glands, as we have seen, have little to do with the formation of red corpuscles, and the oligocythæmia must in this case be referred to failure of the function of the bone marrow. "Anæmia myelogenica," which stands very nearly related to idiopathic anæmia, differs from it in the fact that the marrow shows a lymphatic hyperplasia, which, as I have pointed out, must interfere with the blood-forming capacity of the marrow. That cases have begun as idiopathic anæmia, and later passed into leukæmia, lends, I think, support to the view I take. These blood diseases associated with changes in the cytogenic organs, have, as Dr. Pye-Smith has pointed out, much in common with idiopathic anæmia. Many of the symptoms—the frequency of hæmorrhages, irregular pyrexia, their severity and ingravescence, the resistance to preparations of steel, and the more or less marked influence of arsenic, and the almost invariably fatal issue—are common to both.

Thus I consider all cases in which anæmia is not symptomatic should be regarded as idiopathic. The term "idiopathic anæmia" was, as is well known, first employed by Addison for that particular form of anæmia of which he gave such a masterly descrip-

tion. Others besides myself* urged that it should be restricted to the class of cases described by Addison. The progress of our knowledge, however, in my opinion, now renders it advisable to enlarge its meaning so as to embrace all primary anæmias, and to use it as a generic term, including several species. Of these species two are well defined—chlorosis, and the form described by Addison, which has by common consent come to be called "pernicious anæmia," and which name, I now think, it is useless to oppose. Probably, besides these two species, there are other distinct forms of idiopathic anæmia to which I shall have to allude in subsequent lectures.

LECTURE II.

Secondary Anæmia.—Paroxysmal Hæmoglobinuria.—The Symptoms of Anæmia.—Oligæmia.—Oligocythæmia.—Degrees of Anæmia. Alterations in the Blood Corpuscles.—Hæmorrhages.—Circulatory Organs.—The Urine.—The Alimentary System.—Pyrexia. —The Nervous System.—Chlorosis.—Pernicious Anæmia.

Before proceeding to describe the symptoms of anæmia, I had intended to review the nature of secondary anæmia. The extent of my subject, however, is so wide that I am obliged to omit what I had prepared, and only allow myself to discuss one form of secondary anæmia. This, which has important bearings on many of the debated points in the pathology of anæmia, is paroxysmal hæmoglobinuria. This particularly interesting and important disease is of a purely hæmolytic nature, which lends itself to the study of the blood changes. As is well known, the attacks are excited by cold. To Dr. Copeman we owe some exceedingly valuable information on the changes in the blood in this remarkable disease. He has shown† that in intentionally induced attacks there may be a loss of from 129,000 to 824,000 red corpuscles per cubic millimetre in the course of two or three hours, and that as many as 824,000 corpuscles per cubic millimetre on one occasion were destroyed in a quarter of an hour. At

* 'Lancet,' 1878, vol. ii, p. 797.

† Bristowe and Copeman, 'Trans. Medical Soc.,' 1889, p. 256. Copeman, 'Pract.,' Sept. 1890, p. 161.

the same time the percentage of hæmoglobin fell from 53 to 49. Dr. Copeman calculates that the reduction of red corpuscles in an attack reached the extraordinary number of 4,032,000,000,000. The blood-forming powers in this disease are exceedingly active. In one attack, watched by Dr. Copeman, the corpuscles were reduced from 3,612,500 to 2,930,000, and at the end of six days had risen again to 3,540,000, per cubic millimetre. Still further important changes were observed in the arrangement and forms of the red corpuscles, and in the rapidity with which these changes took place. In the course of from half an hour to two or three hours the *rouleaux* formation was partially or entirely lost, and many red corpuscles had become pyriform or spindle-shaped, and some were nearly colourless. The specific gravity of the blood was also observed to fall about 5° in an attack. The fact that such rapid changes in the forms of the red corpuscles may take place by a process of hæmolysis is of extreme importance, and goes to show that the poikilocytosis met with in many other forms of anæmia may be due to destructive changes. As Dr. Hunter has pointed out,* the appearance of hæmoglobin in the urine in the disease is due to the destruction of corpuscles taking place in the general circulation, whence the hæmoglobin is conveyed to the kidneys and excreted. In pernicious anæmia the destruction takes places within the portal circuit, and all the hæmoglobin is converted into biliary and urinary pigments, and none is excreted as such by the kidneys.†

The Symptoms of Anæmia.

Whenever hæmorrhage of any more than the most trivial degree exists, there must necessarily be diminution in the volume of the blood—oligæmia. The water is quickly replaced by the plasma from the lymphatic system, the deficient albumen and salts are soon supplied from the food, but the corpuscular deficiency remains for a time. In hæmorrhage it is the red corpuscles which are especially deficient, as owing to their "wall-loving" propensities fewer colourless corpuscles are shed; and, moreover, as Cohnheim

* 'Lancet,' vol. ii, 1888, "An Investigation into the Pathology of Pernicious Anæmia."

† I should have liked to allude to the detailed and careful observations of Dr. Rutherford Macphail, "On the Blood of the Insane," 'Journ. of Med. Science,' 1884 and 1885, had space permitted.

points out, the lymph is an inexhaustible source for these. In experimentally induced anæmia in the dog, the number of red corpuscles continues to fall for some days after the hæmorrhage, and does not reach its minimum until eight or nine days. Successive hæmorrhages at intervals are more dangerous than a single hæmorrhage exceeding them in amount, apparently by interfering with the renovation of the blood. How great a loss of blood the human subject can bear is not ascertained, but dogs do not survive a greater loss than from one-nineteenth to one-fourteenth of their body weight.

Oligocythæmia.—In nearly all forms of anæmia the red corpuscles are diminished. The only form of primary anæmia in which this does not necessarily occur is chlorosis. The greatest reduction of the number of red corpuscles is in pernicious anæmia, in hæmorrhage and purpura, and in certain cases of malignant growths. In a case of puerperal hæmorrhage of Hayem's, which recovered, the corpuscles were as low as 850,000 (17 per cent.), and many of these were wanting in colour. Quinquaud has recorded a case of purpura with a corpuscular richness of only 740,000; Hérard one in which it fell to 620,000 on the day of death; and I have published a case in a child 11 months old in which the corpuscles were only 290,000 per cubic millimetre, or 5·8 per cent., just before death.* What is the lowest number of corpuscles which may be reached without death resulting? With the exception of a case of Quincke's—to be subsequently alluded to—the lowest that I know of is one cited by Laache, a case of pernicious anæmia, in which the red corpuscles were reduced to 407,480 (8·1 per cent.), and later rose to 4,148,000 per cubic millimetre at the time of publication. I think we may fix 7·5 per cent. as the lowest which can be reached without being necessarily fatal.

It is convenient in practice to have some scale or measure of degrees of anæmia. For some years past† I have been in the habit of using the following measure of the severity of the anæmia:—Anæmia commences with red corpuscles at 80 per cent. It is moderate at from 80 to 65 per cent., first degree; it is decided at rom 65 to 50 per cent., second degree; it is grave at 50 per cent., third degree; and usually fatal at about 10 per cent (7·5 actually),

* 'British Medical Journal,' 1883, vol. ii, p. 409.

† 'Lancet,' 1883, vol. ii, p. 1047; 'Ophthal. Soc. Trans.,' 1884, p. 132.

fourth degree. My original scale was 65 per cent. moderate, 50 per cent. grave, 35 per cent. very grave, and 20 per cent. or thereabouts fatal. Increase of knowledge has compelled us to shift the last number to a considerably lower level. Though the diminution of red corpuscles is the most important element in diagnosis and prognosis, it is, of course, modified by the relative hæmoglobin richness. M. Hayem has given us a much more complete and valuable scale, which expresses the functional value of the blood. His estimation takes cognizance of the coloured corpuscles, the hæmoglobin, and the individual worth of the corpuscles, as well as the size of the discs.* He also makes four degrees.

As Hayem points out, the anæmia is less grave when the same amount of hæmoglobin is distributed amongst a large number of small globules than amongst a smaller number of larger globules, as the former must facilitate the globular functions. All observers have noticed the fluctuations which take place day by day in the number and sizes of the red corpuscles.

Size of the Red Corpuscles.—In health we have seen the red corpuscles range between 6·5 and 9μ. In anæmia the range is much greater, from 2·5 to 16μ. The term microcyte is conveniently applied to corpuscles from 2·5 to 4μ, and megalocyte to those whose dimensions range from 9 to 16μ. Both these extremes, and gradations between them, are found in all varieties of anæmia—primary and secondary. Both microcytes and megalocytes are more common in pernicious anæmia than in other forms. Megalocytes, nevertheless, are found in chlorosis, in which disease a large number of corpuscles are smaller than normal.

Eichhorst's Corpuscles.—This term is applied to microcytes of a deep colour and spherical shape, to which Eichhorst first drew attention.† He believed at one time that they were diagnostic of pernicious anæmia; they are certainly common in this form of anæmia, though they are sometimes entirely absent,‡ and they have been found in other forms. These corpuscles are from 3 to 4μ, have not the usual biconcave aspect, and Eichhorst describes them as resembling "droplets of fat tinged with red."

* *Op. cit.*, p. 407.

† 'Centralb. f. d. med. Wissenschaft.,' 1876.

‡ Grainger Stewart ('British Medical Journal,' 1876, vol. ii, p. 40) and others.

Poikilocytosis (ποικίλος, varying in shape, κύτος, cell), the name suggested by Quincke* for the variously deformed red corpuscles, has been generally adopted, but Ehrlich has recently proposed to substitute for it "schistocytosis." The shapes assumed by the corpuscles are very varied: ovoid, pear-shaped, battledore, rod-shaped, bent forms, and irregularly outlined shapes are met with.

Nucleated Corpuscles.—Nucleated corpuscles, similar to what are found in the blood of the embryo, and at all times in the marrow, are occasionally, though rarely, found in the blood in anæmia of different kinds, and are probably evidence of its severity rather than of its nature.

Remarkable fluctuations of opinion have taken place regarding the nature and significance of these abnormal forms. At one time considered as peculiar to pernicious anæmia, increasing observation has shown that they may occur in all forms of anæmia. They indicate that the blood elements have undergone a change, constructive or destructive, but the observations of Boas and Copeman in hæmoglobinuria show we must be cautious as to the inferences we draw regarding their origin. Copeman has pointed out how readily crystals of hæmoglobin are formed in pernicious anæmia, a proof of the increased fragility of the corpuscles.†

The Colourless Corpuscles.—In pure anæmia the leucocytes are not increased in number actually, nor in many cases relatively. Not infrequently, however, at the close of cases of profound anæmia, there is an augmentation of the colourless corpuscles.

Hæmoglobin.—In my first lecture I dealt with the variations which take place in the percentage of hæmoglobin in the chief varieties of anæmia. I will only add that in some cases of cancer the hæmoglobin is reduced disproportionately to the corpuscles. Thus, I have recorded a case of cancer of the stomach where the corpuscular richness was 46 per cent., and the hæmoglobin 30 per cent., and later the corpuscles were 30, and the hæmoglobin 15, per cent.; in another case a corpuscular richness of 50 per cent., with 25 per cent. of hæmoglobin, and at the time of death the corpuscles were 33 per cent. and the hæmoglobin 25 per cent. The worth of the individual corpuscles was thus at one time not more than one-half of the normal. In most cases of purpura of which

* 'Deutsch. Archiv f. klin. Med.,' 1877, Bd. xx.

† 'St. Thomas's Hospital Reports,' 1887.

I have notes the hæmoglobin deficiency has been below that of the corpuscles. In regeneration of the blood, as a rule, the corpuscles increase disproportionately to the hæmoglobin. This is usually well seen in chlorosis, but exceptions do occur.

Hæmorrhages.—In anæmia there is a liability to hæmorrhages, external and interstitial. Epistaxis, menorrhagia, and bleeding from the gums are the most common forms of external hæmorrhage, but hæmorrhages occur also from the stomach and bowels. Of interstitial hæmorrhages, extravasations into the retina are the most frequently seen, though I suspect, if we could see the brain, they would be found there equally frequently. Less common than retinal hæmorrhages are those into the skin—purpura. At one time retinal hæmorrhages were regarded as diagnostic of pernicious anæmia; but Quincke, Litten, myself,* and others have recorded cases where retinal hæmorrhages occurred in symptomatic anæmia. It may, I think, be now regarded as established that hæmorrhages in connection with anæmia are dependent upon its degree irrespective of its nature, and that the tendency to hæmorrhage occurs when the red corpuscles are below 50 per cent., and is more dependent on the deficiency in the number of corpuscles than in the amount of hæmoglobin.

Circulatory Organs.—In anæmia of moderate severity the heart's impulse is often diffused and of a jarring character, but in extreme degrees it becomes feeble and often imperceptible. Increased area of dulness, indicating dilatation, is found in some cases, and there is no doubt that anæmia *per se* may give rise to enlargement of the cardiac chambers, as in a case of pernicious anæmia I have recorded.† Murmurs are frequently heard over the cardiac area. Dr. Kingston Fowler‡ has given a very complete account of these, and with his observations my own in the main agree. Eichhorst§ states that in three out of seven cases of pernicious anæmia examined by him a diastolic murmur was heard, and refers to other published cases. He states it may be transitory or persist

* 'Ophth. Soc. Trans.,' vol. iv, p. 132.

† 'Lancet,' 1878, vol. i, p. 13.

‡ 'On the Origin of Anæmic Murmurs.' Churchill, 1884.

§ 'Die progressive perniziöse Anæmie,' 1878, s. 224. Since this was in type I have seen an important paper by Dr. Handford in the 'Intern. Journ. of Med. Sci.,' December, 1890, "On the Condition of the Heart in Anæmia," which deserves attention.

till death, when the aortic valves may be found competent. Dr. Ernest Sansom thus explains such cases:—*

"I have occasionally found murmurs in anæmic subjects closely resembling a double aortic—that is, systolic and diastolic. In such cases you will find that the systolic is the hæmic *souffle* heard at the aortic base, and generated either in the aorta, the subclavian artery, or in the pulmonary artery; and the seeming diastolic is really a continuous venous hum generated in the great veins of the neck. This continuous hum is overpowered during the systolic period by the arterial murmur, but it is audible during the diastole, so the murmur appears to and fro."

Schrwald† gives a somewhat similar explanation. The arterial tension, contrary to what would be expected, is sometimes increased in anæmia (Broadbent, Sansom, Bramwell), and Dr. Broadbent is inclined to attribute to arterial spasm a share in the production of the dilatation of the left ventricle often encountered.‡ My own experience corresponds with that of Dr. Byrom Bramwell, that the arterial tension is only increased in cases of slight or moderate anæmia,§ or, I may add, anæmia of rapid occurrence. In profound anæmia the tension is low, and the artery is empty between the beats. The increase of tension occasionally met with is no doubt of nervous origin. Cohnheim|| has shown that anæmia is an exciter of the vaso-centre, and acts more vigorously in proportion to its degree and rate of progress. The low arterial tension in the late stages of pernicious and other grave forms of anæmia is probably due to the fatty degeneration of the left ventricle, and possibly to exhaustion of excitability of the vaso-motor centre. To the former cause is also due the dropsy which usually occurs towards the close of severe anæmias, though an hydræmic condition of the blood no doubt favours it. The œdema of anæmia is slight in degree, and usually limited to the feet and legs. Nothing like anasarca is ever seen, but dropsical effusions of moderate amount are found in the serous cavities in many fatal cases. Œdema of the feet and ankles is not uncommon in chlorotics who

* 'Diagnosis of Diseases of the Heart,' p. 134.

† 'Deutsch. med. Wochensch.,' 1889, No. 21.

‡ 'The Pulse.' Cassell and Co., 1890, p. 160.

§ 'Diseases of the Heart.'

|| "Lectures on General Pathology," 'New Sydenham Soc. Trans.,' vol. i, p. 428.

are much on their legs, and disappears when they are put to bed, a proof of its dependence on cardiac feebleness. Palpitation and breathlessness are explained partly by the cardiac feebleness and in part by the deficiency of oxygen carriers in the blood.

Pyrexia.—Occasional and irregular pyrexia is common in anæmia of whatever nature. I have found it in traumatic anæmia in previously healthy persons and after hæmatemesis, and in my notes of cases of chlorosis I find slight degrees of pyrexia very common, "a mimic reproduction of the irregular 'anæmic fever' that appears in pernicious anæmia" (Coupland). It is more pronounced in the latter than in other forms, but some cases of pernicious anæmia are throughout apyrexial. The fact that fever occurs in chlorosis is important, as its occurrence in pernicious anæmia, in concurrence with the exacerbations of other grave symptoms, has been regarded as evidence of blood destruction.

The *urine* supplies us with most valuable knowledge as to the changes which are taking place in the blood, and becomes our chief source during life of information as to whether hæmolytic influences are at work. We shall deal with this more fully under pernicious anæmia. Albuminuria is not uncommon in the late stages of severe anæmia, and is best explained by Cohnheim's theory of degeneration of the glomerular epithelium due to the defective quantity and quality of the blood passing through the glomerular tufts.

The Alimentary System.—Functional and organic diseases of the digestive system are common in all forms of anæmia, and may stand in the double relationship of cause and effect. Anorexia, nausea and vomiting, and gastralgia are exceedingly common, the latter, sometimes associated with gastriectasis, being often very marked in chlorosis. Constipation is present in some cases, diarrhœa in others, the latter often being encountered in cases of pernicious anæmia.

The *nervous system* shares in the general disturbance. Sustained mental activity is rendered impossible, and an unstable condition or "irritable weakness" of the nervous centres is induced, which shows itself in hysterical phenomena in some cases of chlorosis. In profound chronic anæmia, as in the acute anæmia of severe hæmorrhage, delirium and convulsions may close the scene, or be followed by coma. In anæmia, especially in its primary, but not very profound, forms, neuralgic affections are very common, ex-

plained by the late Dr. Anstie as "the cry of the nerves for healthy blood." Amaurosis, from optic neuritis and consecutive atrophy, occurs in some cases of anæmia, especially that following hæmorrhage.

We must now pass on to consider the principal species of idiopathic anæmia. In doing this, my object is rather to review the facts regarding them in relation to their pathology than to give a complete clinical picture, which the time at my disposal does not allow of. First of all we must consider

Chlorosis.

In this form of anæmia the observations of Duncan* in two cases have in the main been confirmed by all subsequent observers, namely, that the essential feature in the blood change is a diminution in the hæmoglobin capacity, an oligochromæmia. In many cases the red corpuscles are scarcely at all below the normal, whilst the hæmoglobin is reduced to from 60 to 30 per cent. In other cases the red corpuscles are also deficient, but in these also the hæmoglobin is disproportionately reduced. Thus, two categories of cases are found in chlorosis, to the first of which Laache† would give the name "pseudo-chlorosis," while the second he would call "true chlorosis." What is common to both is that the corpuscular worth is diminished. Whilst, however, this is characteristic of chlorosis, it is not peculiar to it. In severe cases of chlorosis, the corpuscles undergo a notable diminution, constituting what some would designate chloro-anæmia. Thus Laache has published a case in which the corpuscles were as low as 2,440,000 (48·8 per cent.), Coupland a case with 2,090,000 (41·8 per cent.), Gowers a case with 26 per cent., whilst M. Hayem records a case in which the actual number of red corpuscles was 937,360 (18·7 per cent.). Another characteristic of chlorosis is the relatively large number of small red corpuscles of 3μ to 6μ, but they may be reduced to even $2{\cdot}2\mu$ (Laache). Though some large or even giant corpuscles are occasionally found in chlorosis, Malassez stands almost alone in stating that, as a rule, the corpuscles are increased in size in chlorosis. Poikilocytosis is sometimes present, as in the diagram exhibited, which is taken from a girl aged 18, with 85 per

* 'Sitzungsbericht d. k. k. Acad. d. Wissensch. in Wien,' Bd. lv, 1867.

† *Op. cit.*, s. 86.

cent. of red corpuscles, and 35 per cent. of hæmoglobin. In chlorosis there is little liability to hæmorrhage, which we have seen stands intimately related with severe degrees of oligocythæmia. This applies to retinal hæmorrhages, which, apart from inflammatory changes, are certainly rare. Papillitis and papillo-retinitis are, however, probably more common in this form of anæmia than in any other. Saundby and Eales* found that, of fifty cases of chlorosis examined by them, 10 per cent. presented unequivocal evidence, and in a further 8 per cent. there was evidence of some degree of neuro-retinitis. Hirschberg was the first to note the occurrence of optic neuritis in chlorosis. Gowers has recorded several cases, and cases of double papillo-retinitis have been recorded by Mr. R. Williams† and myself.‡ In my own case, which is one of several I have seen, the corpuscular richness was 80 per cent., and the hæmoglobin only 30 per cent.

The existence of hypermetropia, as first pointed out by Gowers,§ increases the liability to inflammatory changes. Cardiac murmurs and venous hum in the neck constitute part of the clinical features of chlorosis with which all are familiar, and with them are associated palpitation and breathlessness on exertion. In speaking of the symptoms of anæmia in general, I have remarked that pyrexia is by no means uncommon in chlorosis. In some cases the pyrexia is considerable, constituting what has been called "febrile chlorosis." Hayem stated that chlorosis only assumes a febrile form when the globular richness, expressed in healthy corpuscles, is below $1\frac{1}{2}$ million. With regard to the urinary system, most authorities state that it is normal in amount, pale in colour, of low specific gravity, and contains but little urea and uric acid, and my own experience agrees with this. Dr. Hunter has shown that the urine in chlorosis in three cases he examined contained only 1·76 milligrammes of iron, whilst that of health averages 5·65 milligrammes per diem.‖ Further observations on the urine in chlorosis, clinical and chemical, are greatly wanted. The alimentary system very frequently, though not invariably, shows signs of disturbance, and sometimes these are so pronounced

* 'Ophth. Rev.,' i, p. 303.
† 'British Medical Journal,' 1884, vol. i, p. 10.
‡ *Ibid.*, 1885, vol. i, p. 328.
§ 'Medical Ophthalmoscopy.'
‖ 'Med. Soc. Trans.,' vol. xiii.

that some would make a variety, "dyspeptic chlorosis." How far they are caused by the anæmia and how far by the conditions that give rise to chlorosis are much debated questions. Constipation is exceedingly frequent, and is believed by many to be causally related to chlorosis. Its absence in many cases and the frequency with which constipation occurs in young people who are not anæmic make the dependence of chlorosis upon constipation very doubtful, and I believe its importance has been considerably overrated. The nervous disturbances—"the irritable weakness"—of chlorosis are very characteristic phenomena, and to be explained partly by the anæmia, and in part by the conditions that give rise to it. The age at which it occurs and the sex of the sufferers imply a highly sensitive and mobile, if not unstable, condition of the nervous system. As well known, chlorosis is frequently associated with menstrual irregularities, especially amenorrhœa; but it also occurs in those whose catamenia are regular, if somewhat scanty, and may coexist with excessive menstrual discharge. Leucorrhœa is very common in chlorosis, disappearing with its subsidence.

Chlorosis occurs almost exclusively in women, though occasionally cases are met with in young males. It is met with especially between the ages of 14 and 24. All are familiar with Niemeyer's oft-quoted statement: "According to my observation, obstinate chlorosis attacks all young girls, without exception, in whom the menses have appeared in the twelfth or thirteenth year, and before the development of the breasts and pubes."* It is occasionally hereditary, and possibly congenital. The narrowing of the aorta, the thinning of the arterial tissues and irregularities of position of the arterial branches, described by Virchow, though not invariably present, point to developmental defects. The character of the blood, its poverty in hæmoglobin, comparative richness in other albuminates, the markedly renovating influence of iron, all indicate a defective hæmogenesis, a "hypoplasia" of the blood as well as that of the blood vessels (Immermann). Moreever, there is little or no evidence, clinical or pathological, of excessive hæmolysis. There is no drain upon the system other than that of menstruation, and this, in many cases, is suspended. On all these grounds we are justified in regarding chlorosis as due

* 'Text-book of Practical Medicine,' American translation, 1876, vol. ii, p. 743.

to defective blood formation, often associated with, and possibly in some cases due to, disturbances in the generative system. Most probably both are due to some inherited or acquired developmental feebleness of the blood-forming organs, rendering them unequal to the calls made upon them, which, treated promptly and properly in most cases, is of transitory duration, but which, more profound in degree or imperfectly treated, may prove a lifelong complaint.

Pernicious Anæmia.

I have already given reasons which appear to me sufficient for adopting this name for another variety of idiopathic anæmia. First described by Addison, in his lectures in 1843, which were published in 1853, a knowledge of the disease was confined to a few, until Gusserow, in 1871, and Biemer, in 1872 (the latter calling it "progressive pernicious anæmia") drew attention prominently to the subject. Since then a large number of writers at home and abroad have contributed to our knowledge of the disease.

It is impossible to allude to the work of most of these in the time at my disposal, but I must mention the service Dr. Pye-Smith has done to medical literature in his articles in the 'London Medical Record,' 1877, and in 'Guy's Hospital Reports,' vol. xxvi, in which will be found historical and critical reviews of the literature, and valuable original observations and pathological teaching. I must also allude to Dr. F. Taylor's paper, in 'Guy's Hospital Reports,' and, since this was in type, to a paper in a forthcoming number of the same Reports, by Dr. Hale White, of which he has allowed me to see a proof, to Dr. Sidney Coupland's able and complete Gulstonian Lectures on anæmia, in 1881, and to Dr. William Hunter's numerous and valuable contributions on the subject. Dr. Hunter's clinical, chemical, pathological, and experimental investigations, carried on over a series of years, have, by the ability with which they were conceived, and the exactness with which they have been carried out, added more to our knowledge on the subject than those of any individual worker since Eichhorst's valuable treatise in 1878.

Pernicious anæmia manifests itself insidiously by increasing pallor and weakness. In a large proportion of cases disturbances of the alimentary canal (nausea, vomiting, diarrhœa, &c., and not

infrequently stomatitis) precede or accompany the onset of the disease, and such disturbances are of common occurrence throughout its course. The patient, whilst losing colour and strength, maintains, as a rule, his flesh. Since Addison's time the details of the anæmia, which is so striking to the naked eye, have been most carefully investigated by means of modern instruments of precision.

I have already pointed out that the specific gravity of the blood is reduced from 1038 to 1028, and that its alkalinity is lessened. In this form of anæmia the solids of the plasma are greatly diminished. Great attention has naturally been devoted to the red corpuscles, and there have been noted the following alterations :— (*a.*) Their cohesiveness is modified; normal *rouleaux* formation is rarely seen; usually the corpuscles are arranged singly, or collected into clumps, consisting of a few red corpuscles irregularly grouped together. (*b.*) The reduction in the number of the corpuscles is remarkable, and only equalled by cases of hæmorrhage and rare cases of malignant growths, and only exceeded by cases of fatal spontaneous hæmorrhage, such as purpura with epistaxis. At a time when the disease is recognisable, the number is below 50 per cent., and generally at or below 30 per cent. ($2\frac{1}{2}$ to $1\frac{1}{2}$ millions per cubic millimetre). In fatal cases the numbers at the close of the case are extraordinarily low. Thus Lépine has recorded a case with 378,000 per cubic millimetre (7·5 per cent.) the day before death, Worm-Müller with 360,000 (7·2 per cent.). Quincke has published a case in which the corpuscles were reduced to 143,000 (2·8 per cent.) without death resulting, but all Quincke's numbers read much lower than those of any other observer. It may be stated that in fatal cases the corpuscles generally sink to about 18 to 16 per cent. before death, and, with the exception of Quincke's case, no case has been known to recover in which the corpuscles have fallen as low as 7·5 per cent. It will thus be seen that nearly all cases have reached the stage which I have called "grave anæmia" (50 per cent.) when first recognised, and nearly all to Hayem's fourth degree of anæmia, or "*aglobulie extrême*" before death. (*c.*) Poikilocytosis, though, as has been shown, not peculiar to this form, is more constant, and carried to a more extreme degree than in other varieties of anæmia. The most remarkable alterations in the forms of the corpuscles are sometimes observed. (*d.*) Their size presents great varia-

tions. Many microcytes (Eichhorst's corpuscles) are usually seen, and, though not peculiar to, are most common in, this form of anæmia. On the other hand, megalocytes are also present. What, however, is especially characteristic of pernicious anæmia is the large number of red corpuscles above the normal size, but not so large as to really deserve the name of megalocytes —corpuscles of from 8 to 12μ. Nucleated corpuscles have been found by many observers, but are not always present. (*e.*) The vulnerability of the corpuscles is increased. This is shown by the manner in which the colouring matter separates from the stroma, as pointed out by several observers,* and, as Copeman has shown, by the readiness by which blood crystals form from them without the addition of any reagent. (*f.*) The hæmoglobin capacity of the corpuscles is generally increased, so that the percentage of hæmoglobin exceeds the percentage of corpuscles, and the worth of the individual corpuscles is increased, often to 1·25, and occasionally to as much as 1·75 (1·0 = normal).

I have, however, recorded a case† in which the worth of the corpuscles was as low as, on one occasion, 0·75, and, on another occasion, 0·26. In the great majority of cases the worth is increased, so that, as pointed out by Osler, the anæmia is less than is indicated by the deficiency of corpuscles. The fact, however, that the majority of the corpuscles are larger than normal neutralises, to a great extent, any advantage accruing from the increased worth of the individual corpuscles, as the same amount of hæmoglobin distributed amongst a smaller number of large corpuscles must expose a smaller superficies than distributed in a larger number of small corpuscles. The colourless corpuscles are not, as a rule, increased in pernicious anæmia, but a marked leucocytosis has been observed by Litten and Gottlieb, and myself, as a premortal phenomenon. Hæmorrhages are more common in this form of anæmia than in any other, especially due, as I have pointed out, to the oligocythæmia. Retinal hæmorrhages are almost invariably present, and petechiæ are not uncommon. External hæmorrhages, especially epistaxis, bleeding from the gums, and metrorrhagia, are common, and other hæmorrhages are not rare. After death hæmorrhages into the brain and its membranes have

* Byrom Bramwell, 'Edin. Med. Journ.,' 1887; Mackern and Davy, 'Lancet,' 1877, vol. i, p. 642.

† 'Ophthal. Soc. Trans.,' vol. i, p. 50.

been found in several cases, and minute extravasations beneath the serous membranes are not uncommon. In one case I found small hæmorrhages in the liver. The troubles in the circulatory system, characteristic of anæmia, are pronounced, but do not call for special remark. Pyrexia is present in the majority of cases. It is irregular in its course and duration. The temperature often runs up to 101° or 102°, and may be even as high as 104°. It does not remain high long. Exacerbations of most of the symptoms have been noticed to accompany a rise of temperature in some cases, but this is not invariably the case, and some cases of pernicious anæmia are non-febrile throughout their course. I have already alluded to the disturbances in the alimentary system. The skin acquires a peculiar tint—a "faded leaf," or discoloured wax, or a lemon tint, which is very characteristic. The interpalpebral conjunctiva is often yellow, but in some cases is pearly-white. The "lemon tint" has been found to vary from time to time, and an increase to occur at periods when the urine becomes darker and more bile is passed by the fæces, in one case the pyrexia being at the same time increased. In some cases the yellow colour is more marked, and Bartels has collected eleven cases, and added a twelfth of his own,* in which it amounted to a true icterus. In these cases no disease accounting for it was found in the liver; and Bartels attributes the jaundice in such cases of pernicious anæmia to a more acute course than usual; in other words, to an unusual activity of destruction of red corpuscles. Dropsy, to a slight degree, about the feet and ankles, is common towards the close of the disease, but never anything amounting to an anasarca.

The urine presents important changes in many cases which, according to some observers, have great diagnostic and pathological significance. Several observers (Fagge and Pye-Smith, Bristowe, Eichhorst) have remarked that in some cases the urine has a dark colour. This has been shown by Dr. Hunter† to vary from time to time, with aggravations in the other symptoms. He states that the colouring matter is "pathological urobilin," whilst in one case Mott found it was caused by normal urobilin. MacMunn's observations go to show that normal urobilin is, in all

* 'Berlin. klin. Wochenschr.,' 1889, No. 43.

† "Observations on the Urine in Pernicious Anæmia," 'Practitioner,' 1889 and 1890.

probability, derived from the hæmatin of the tissues, and perhaps from the hæmatin of the food, whilst pathological urobilin is derived, in all probability, from the bile pigments, and to a less extent from hæmatin. Connected, as its appearance in the urine seems to be, with increased secretion of dark bile, Dr. Hunter considers that the excretion of large quantities of pathological urobilin is valuable evidence of excessive destruction of red blood corpuscles. He has also found in the urine pigment granules in renal cells and casts in one case, whilst corresponding granules, which gave a blue reaction when treated with a solution of ferrocyanide of potassium, and afterwards dilute hydrochloric acid, in the epithelium of the convoluted tubes of the kidney in all the cases, twelve in number, of pernicious anæmia he had examined. Dr. Hunter has further shown that in pernicious anæmia there is a remarkable increase in the excretion of iron in the urine, a fact which has also been previously pointed out by Dr. Magee Finny, from analyses made by Professor Reynolds.*

So much in the way of inference hangs on the condition of the urine, that I must raise the question whether dark urine, or urine containing an excess of normal urobilin or pathological urobilin, is invariably present in pernicious anæmia. In several cases of my own, with post-mortem examinations confirming the diagnosis, the urine was never noticed to be dark, being "amber" or "straw-coloured" throughout. I find also among recorded cases the urine is stated to have been "pale" in a fatal case by Byrom Bramwell, and by Mott (the latter only one examination); "very pale" in two cases (one fatal) by Bramwell; "natural" in colour in four cases (one fatal) by Bramwell and one by Musser.† My own cases, it is true, were not examined especially with reference to this point of colour, though the many observations made are against it being overlooked. The same remark possibly applies to the other published cases, with the exception of that by Mott, who was one of the first to call attention to the fact that the urine is often of dark colour in pernicious anæmia. Eichhorst, whilst recording a case in which the urine was of dark colour, states it is usually of bright-yellow colour, and that dark (*dunkel*) is exceptional — possibly by "*dunkel*" turbid is meant. In some of Laache's carefully recorded cases the urine was pale in colour. It

* 'British Medical Journal,' 1880, vol. i, p. 44.

† 'Proc. Phil. Co. Med. Soc.,' 1885.

is possible that the cases in which the urine is dark may form a separate category from others in which this peculiarity is wanting, but I especially direct attention to the point that it may be cleared up by future observers.

Dr. Hunter, struck with the similarity of the hæmolytic process in pernicious anæmia with that induced in animals by the action of toluylene-diamine, more especially as regards the limitation of its action to the portal blood, was led to the conclusion that the blood destruction in the former is due to the action of a poison of a cadaveric nature, and that this is probably produced within the gastro-intestinal tract and absorbed from thence. Accordingly he instituted some investigations on this point, and these had as their result a demonstration that there is in pernicious anæmia a marked excess of aromatic sulphates in the urine, derived from the sulphur of the proteids of the food, and that this is indicative of increased but perverted gastro-intestinal activity and absorption. He has been able to obtain from the urine two ptomaines, identical with putrescine and cadaverine obtained from putrefying meat, and a third ptomaine, which appears to be a special diamine body, distinct from putrescine and cadaverine. Dr. Hunter points out that ptomaines belong to the group of nitrogenous bases formed from proteids by the action of bacteria. Ordinary putrefactive organisms do not give rise to ptomaines in the urine, but they have been found in the urine and fæces in cystinuria by Udransky and Baumann, and by Brieger. Neither putrescine nor cadaverine possesses any markedly poisonous properties, but they derive their importance from the fact that they are the common products of bacteria, differing widely in their pathological qualities. But the presence of even non-poisonous ptomaines, pointing to the existence of special micro-organisms in the alimentary tract, has a special significance, in the opinion of Dr. Hunter, as indicating the existence of one condition more than any other favourable to the production of other and more poisonous ptomaines. The symptoms in one case of pernicious anæmia of Dr. Hunter's appear to support such a conclusion. Exacerbations occurred from time to time, presenting the nervous phenomena of toxic poisoning. Dr. Hunter, therefore, feels justified in drawing the conclusion that the special factor required to initiate the symptoms peculiar to the form of anæmia is the presence, under certain favourable conditions, of micro-organisms of specific nature

within the gastro-intestinal tract; and these favourable conditions he conceives to be such as have been met with—malignant disease and such other organic changes as have been found in association with this disease, or functional disturbances of the mucous membrane of the stomach and intestine induced by the presence of intestinal parasites, or by prolonged bad nourishment. Dr. Mott, in a valuable communication to which I have already several times alluded, does not believe that there is always a relation between the pyrexia, the diminution of corpuscles, and the colour of the urine. He points out, as Dr. Hunter has done, that ulceration in the intestines may exist in which the whole contents of the bowel must swarm with micro-organisms of many kinds, with everything to favour absorption of their contents without pernicious anæmia occurring. Further, that though constipation and anæmia may concur, and the latter may be benefited by curing the former, in such cases of fæcal poisoning, the corpuscles are not broken down, but insufficient colouring matter is found.

The morbid changes found after death in pernicious anæmia indicate three processes: 1. The effects of the anæmia, in the bloodless condition of all the organs, with the exception, in some cases, of the spleen; and in the fatty and degenerative changes, especially marked in the heart and blood vessels. 2. The activity of the cytogenic organs, especially the bone marrow, which is usually red and vascular (and containing enormous numbers of developing red blood corpuscles) where it is normally yellow and fatty. 3. The signs of excessive hæmolysis and of the process of removal of the broken down corpuscles; the former, as demonstrated in the liver and spleen, by the evidences of active destruction of the red corpuscles; the latter, in the presence of blood pigment containing iron in the kidneys. Thus the evidence goes to show there is excessive blood destruction and its consequences, which the blood regeneration, however active, is unequal to compensate for. Dr. Hunter's pathology is so coherent, and supported by so much direct evidence, that I am convinced of its adequacy and correctness in some at least of the cases regarded as pernicious anæmia. For cases in which the urine is pale throughout, containing no pathological urobilin and no excess of normal urobilin, and no increase of iron—if such should be proved to occur—another explanation must be found; some different evidence of blood dissolution must be found, or unequivocal evidence afforded

of defective hæmogenesis. Ehrlich remarks: "Degeneration and regeneration course side by side in uncomplicated anæmia; it depends upon the preponderance of one or other process whether this simple anæmia ends in a cure or in the progressive pernicious form."*

LECTURE III.

Scorbutic Anæmia.—Relapsing Pernicious Anæmia.—The Diagnosis of Anæmia.—The Treatment of Anæmia.—Of Defective Hæmogenesis.—Iron.—Of Excessive Hæmolysis.—Arsenic.—Concluding Remarks.

I HAVE already mentioned that there are cases of idiopathic anæmia which differ from cases of chlorosis and the general type of pernicious anæmia. To these I must now direct your attention; and first to cases of

Scorbutic Anæmia.—I have published† a case of a man, aged 18, who, one week before he came under my observation, was attacked with headache and cough, followed by swelling of, and bleeding from, the gums. When admitted to the hospital, the gums were much swollen, bleeding, and ulcerated; petechial hæmorrhages existed nearly all over the body; no subcutaneous hæmorrhages or synovitis. Urine free from blood and albumen. Examination of the blood showed C. 51, H. 27, W. 0·52.‡ In the last two years he had had meat and potatoes every day, and green vegetables once a week. I was, nevertheless, inclined to regard the case as scorbutic at first, but in spite of all treatment, including anti-scorbutics, he became more and more anæmic, but did not lose flesh; retinal hæmorrhages, more profuse than in any other case I have ever seen, developed, and ultimately general retinal œdema. The cutaneous hæmorrhages disappeared, but the gums remained spongy, and the bleeding from them considerable. Frequent and distressing vomiting set in, murmurs developed over the heart, and the pulse became exceedingly feeble. The urine contained a

* 'Charité Annal.,' 1888.

† 'Ophthal. Soc. Trans.,' vol. i, p. 51.

‡ For brevity the following signs are used: C. = red corpuscles per cent.; H. = hæmoglobin per cent.; W. = worth or value of individual corpuscle.

trace of albumen throughout after the first examination, but no œdema occurred. The red corpuscles sank to 13 per cent.; they varied much in size, some presenting cracks and fissures. The disease lasted a little over two months. The necropsy showed some thin hæmorrhagic extravasations beneath the pia mater; brain exceedingly white; lungs œdematous, with a few hæmorrhages. Numerous ecchymoses on surface of heart; muscle of left ventricle pale and mottled; liver and spleen anæmic; stomach very anæmic, ecchymosed; solitary glands, especially of large intestine, prominent; mucous membrane pigmented. Two small ulcers in large intestine, with deeply pigmented borders. Mesenteric glands mostly healthy, but several enlarged, and filled with calcareous matter. Micrococci were found in the capillaries of the kidneys, liver, and heart. They were only in small colonies, and there were no important changes in the tissues around them. It would, therefore, appear probable that they gained admission to the blood only shortly before death, increasing probably after death. The patient had suffered recently from primary syphilis, but no signs of visceral syphilis were found in the body. I have also recorded* another case somewhat resembling it, but running a more acute course. Dr. Gowers has recorded† two cases (one fatal) of a similar nature, in which the patients had plenty of vegetables, but had abstained more or less from animal food. He has suggested the very approximate name of "scorbutic anæmia" for such cases.

I next wish briefly to relate a couple of cases which do not conform in all respects to the type which some recent writers have given of pernicious anæmia, particularly as regards the urine, and which show the marked tendency of this form to relapse.

Relapsing Pernicious Anæmia.—Case 1.‡—S. F——, first admitted under my care on July 9th, 1884. He was an engineer, aged 39, employed sometimes on ships and sometimes on land on stationary engines. He never had ague or syphilis. In 1879 he went to work in the Severn Tunnel. There he suffered much from the defective ventilation and the fumes of dynamite used for blasting purposes. He stated that there were as many as fourteen deaths from these causes. After remaining there nine months he returned to Newport, where he did a great deal of "over-

* 'Ophth. Soc. Trans.,' vol. ii, p. 34.

† 'Medical Ophthalmoscopy,' 3rd ed., p. 246.

‡ Compiled from notes by Messrs. Owen and Kenneth Morell Mackenzie and Drs. Taylor and Robb.

time" work, often working for four nights in succession, besides doing his day's work. In December, 1880, he noticed he was losing colour, and this was followed by loss of appetite, and some wasting. He went into the Newport Infirmary, and after three months' treatment was able to resume work. He had a succession of break-downs and recoveries from that time, and his case excited a great deal of interest; a number of doctors visited him on account of the peculiarity of his case, and he was not expected to recover. He was sent to me by Dr. W. F. Edwards. He was temperate in everything except work. His attacks appear to have come on rapidly, in varying circumstances, and, after lasting an indefinite period, to have comparatively rapidly passed off. To use his own words, "When I begin to get well I get well quickly."

On admission he complained of great weakness and depression. There was extreme pallor, and a faint-yellow tint of the skin. His pulse was 96, tension not increased; systolic murmur over third left rib, conducted to apex; no dilatation, no retinal hæmorrhages, no œdema. His blood, on admission, showed C. 13, H. 20, W. 1·5. His urine had specific gravity 1021, and was of amber colour. His history of working in a tunnel led me to prescribe anthelmintics and to examine the stools microscopically, but with negative results. He was troubled with constant nausea and occasional vomiting at first, but there was at no time diarrhœa. He was put on Blaud's pills, which he took for some time without benefit. Arsenic was then substituted, at first in 4-minim doses, increased later to 6, three times a day. The improvement, as shown by the chart, was immediate and progressive from the time he took arsenic, and when discharged, on September 2nd, examination showed C. 95, H. 90, W. 0·94. His urine was examined every few days throughout his residence in the hospital. It was "straw-" or "amber-" coloured, except on three occasions, when it was "orange." These were, when he was rapidly improving, C. 55, H. 33, W. 0·6. The average quantity was 65 ounces, and the specific gravity varied from 1010 to 1018. His temperature never exceeded one degree above the normal. He was admitted a second time in the early part of 1885, under Dr. Fenwick, and recovered under arsenic.

Third admission, November 14th, 1885. This relapse began with nausea, anorexia, and rapid loss of colour and strength two months before. He had vertigo, faintness, and palpitation on the least exertion. He had been under medical treatment six weeks, but the only result, he said, was constant diarrhœa the whole time. He had complete anorexia, discomfort after what he took, and looseness of the bowels. No *bruit* over heart, venous hum in neck. Defect of vision in right eye, with some atrophy of papilla. Loss of smell. No œdema. Blood on admission C. 20, H. 38, W. 1·9. It was, later, C. 25, H. 24, W. 0·96; and then steadily rose, when taking arsenic and iron, to C. 73, H. 86, W. 1·17. His urine, on admission, was of deep colour, 1018, alkaline; with this exception it was "amber" or "deep amber" on each occasion. It was deep amber most frequently when he was recovering. It contained a trace of albumen on four occasions. He left on January 12th, 1866, feeling well, and with a good colour. His temperature was 100° F. several times, and once rose to 102·5°. It will be observed that the recovery, as indicated by the condition of the blood (C. 73, H. 86, W. 1·17), was not so complete on this as on other occasions. This I attribute to arsenic not being used so freely. He took mist. ferri arsenicalis, which contains 5 minims of Fowler's solution, and pil. ferri co. three times a day.

Fourth admission, March 31st, 1887. This relapse had existed about

one month. It was apparently induced by a cold, following which he had the usual train of symptoms—nausea, anorexia, anæmia, and some wasting; he was very pale; there was anorexia, breathlessness, and palpitation on the least exertion. No murmur. His blood, on admission, showed C. 45, H. 40, W. 0·88; on April 5th, C. 34, H. 45, W. 1·35. It improved under arsenic a little irregularly to C. 95, H. 80, W. 0·84. On the last two examinations the corpuscles exceeded the hæmoglobin. The urine averaged 56 ounces; the specific gravity varied from 1012 to 1020. The colour was "pale," "yellow," or "amber" on every occasion except one, when it was "dark sherry." This was when he was improving (C. 58, H. 65, W. 1·1). There were occasional cases of peptones and albumen; slight deposits at times of uric acid and phosphates, and indican was present twice. His temperature never exceeded 100° F. He was treated with Fowler's solution, beginning with 5 minims, and increasing gradually to 10 minims three times a day.

Fifth admission, May 31st, 1888. The attack had lasted one month, came on in the usual manner, and had the same general characters. He was at once put on arsenic, beginning with 5 minims and increasing to 12, three times a day. The corpuscles, which alone were measured, were 37·6 per cent., fell to 21 and 15 per cent. later, and only reached 41 per cent. when discharged on September 4th. He remained weak for one month, and then became worse and died in a week with delirium and vomiting.

Case 2.*—I have another very similar case under my care at the present time—D. M——, boiler maker, aged 48. He had increasing pallor, weakness, with loss of flesh, for many months before admission on December 6th, 1889. There was great pallor, anorexia, and costiveness; pulse 84, small and soft; no murmur, but venous hum in neck. Hæmorrhages in both retinæ.

On admission, C. 30, H. 25, W. 0·83. He was put on arsenic, beginning with 5 minims, and increasing to 13 for a dose three times a day. He rapidly gained colour, strength, and flesh, and the retinal hæmorrhages disappeared, and on discharge he had C. 82, H. 68, W. 0·82. His urine was amber, pale amber, and straw-coloured throughout; its density varied from 1014 to 1028, and was always free from albumen. His temperature was never above 99° F. His weight increased from 9 stone to 10 stone 5½ lbs.

Second admission, November 28th, 1890. His appetite had been bad all through the summer, and he had consequently lost flesh. In the last five weeks appetite almost entirely left him, he became very pale and weak, and suffered from constipation and headache. He was very pale on admission, and his skin and conjunctivæ had a yellowish tint. First sound of heart feeble and distant, second reduplicated; dyspnœa on the least exertion; no hæmorrhages in retina; C. 46, H. 30, W. 0·65. He was put on arsenic solution, beginning with 5 minims and increased by degrees to 12 three times a day, with a morning salient aperient. He has steadily improved, and his blood on January 15th was C. 75, H. 60, W. 0·8. His urine has been examined daily. It has averaged 58 ounces, specific gravity from 1018 to 1030. It has never contained sugar, albumen, or

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* From notes by Messrs. Turrell, Lea, and Stanton.

bile pigment. Particular attention has been paid to its colour. It has been "amber," "yellow," or "pale straw" on most occasions, "darkish straw" or "pale brown" on several. It has never contained any excess of normal urobilin, nor any pathological urobilin, though examined daily for these. He has had one febrile attack, in which his temperature reached 102·5°, lasting two days.

These two cases present many points of interest, two only of which I must dwell upon. First, the character of the urine. In Case 1 it will be observed that it was always pale, except when he was recovering, that is, when the pathological process causing the anæmia was least active, at a time when he was taking arsenic. I believe I have observed the same effect in some other cases, the urine becoming dark when the patient was being treated by arsenic. Arsenic, as well known, is eliminated by the kidneys.

In Case 2 the urine has been examined most carefully daily by my house physician, Dr. David Brown, who is a very good chemist; and, though it has been darker on some occasions than others, there has been no pathological urobilin, and no excess of normal urobilin.

The second point to which I must refer is the prognosis in such cases. In Case 1, repeated recoveries under arsenic were followed by repeated relapses over a period of about nine or ten years, and at the earlier part of the illness he was given over as irrecoverable. Change of locality, change of occupation were tried without avail in warding off the fatal termination. Though Case 2 is now nearly convalescent, I have little doubt he will have a return of his disease with the same ending.

Dr. Hale White, in a paper in the forthcoming vol. xlvii of 'Guy's Hospital Reports,' of which he has allowed me to see a proof, has gone very carefully into the prognosis of cases of pernicious anæmia, and shown that, though a small number of cases left Guy's Hospital relieved, several relapsed and died of the disease in the same hospital. Moreover, during the ten years 1879 to 1889, there were nine patients believed to be suffering from pernicious anæmia who recovered sufficiently to leave the hospital. Of these two are known to have died, and, from reasons given in the paper, there is a presumption that six more died. One case under Dr. Hale White's care recovered and remained well at the end of nearly four years. A previously published case of Dr. Pye-Smith's was well at the end of two years. These are the only two in which a real recovery can be stated to have occurred in Guy's Hospital

since the time of Addison. Both were treated with arsenic. Dr. Tyrrell Brooks has recorded a case* of apparent recovery, but with a fatal relapse a year later. I can endorse Dr. Hale White's statement that a subsequent report of cases published as cured by arsenic is much to be desired.†

Diagnosis.—The diagnosis of anæmia presents no difficulty if we make this reservation, that pallor is not always indicative of the presence of anæmia. The colour of the mucous membranes is a more trustworthy guide than that of the skin. In Bright's disease, for instance, patients look much more anæmic than is proved to be the case. Having established, by actual examination of the blood, that anæmia exists, we have next to determine on what this depends; in other words, whether it is a secondary and symptomatic, or a primary and idiopathic, anæmia. To exclude the former we have to consider the history of the case, and to make a careful and complete physical examination. There are in this country two diseases of which anæmia is symptomatic, and in which the signs of local disease may be wanting for some time; these are tuberculosis and cancer. Anæmia may be one of the earliest indications of pulmonary and other forms of tuberculosis. It is usually, but not invariably, accompanied by wasting. The occurrence of anæmia and wasting in a young person should excite a suspicion of incipient tuberculous disease, and lead to repeated examination of the lungs and other organs. Wasting is not characteristic of chlorosis, but may accompany it, and be carried to an extreme degree, as in a case of chlorotic hysteria sent to me by Dr. Heywood Smith in 1882. The patient was emaciated to an extreme degree. When first seen she had C. 50, H. 30, W. 0·6; later, C. 48, H. 20, W. 0·42, and ultimately rose to C. 96, H. 60, W. 0·62. Her weight when she first came under my care was 4 stone 2 lbs., and it rose to 6 stone 7 lbs. She has remained well ever since, but has entire suppression of menses, and has become exceedingly stout. I was once present at a post-mortem examination of a case believed, during life, to be pernicious anæmia, but which was proved to be cancer of the stomach. In this case the

* Reprint from 'Lancet,' 1887, vol. ii, p. 859.

† Laache, at the International Congress, 1890, narrated a case in which under arsenic rapid improvement took place, an increase of about 211,000 per cubic millimetre taking place daily, and the patient appeared to recover completely; after some months he relapsed and died.

diagnosis was rendered peculiarly difficult by the entire absence of gastric pain and vomiting. This case shows the caution necessary in diagnosis, and in accepting as pernicious anæmia cases not proved by necropsy. Whilst indeed it was necessary at one time to reject all cases not proved by post-mortem examination, so as to get a clear picture of the disease, yet we are not justified now in rejecting cases which have the characters I have described. It must be remembered that neither variations in the size, nor alterations in the shape of the red corpuscles, are peculiar to idiopathic anæmia. Still, the careful examination of the blood will go far towards the diagnosis of the variety of anæmia. Thus the reduction of the red corpuscles to, or below, 30 per cent., especially if the hæmoglobin is relatively high, is in favour of pernicious anæmia. The other diseases in which such great reduction in the number of corpuscles occurs are hæmorrhage, in which the corpuscles and hæmoglobin usually have their normal relationship, and the history of loss of blood is rarely wanting, and cancer, in which, as in some cases I have mentioned, the hæmoglobin is sometimes greatly below the corpuscular richness. In chlorosis the marked characteristic is the deficiency of hæmoglobin. It should be further remembered that retinal and other hæmorrhages, whilst characteristic of pernicious anæmia, are not pathognomonic of it. They indicate a severe degree of oligocythæmia—usually less than 50 per cent. of red corpuscles. A rough, but serviceable, guide to the recognition of this degree of anæmia is furnished by the colour of the nail-bed of the fingers. As long as any pink colour can be seen, then it may be assumed that there is at least 50 per cent. of red corpuscles. When the pink colour entirely disappears from the nail-bed, the corpuscles will be found to be below 50 per cent. The recognition of this degree of anæmia is a warning that the grave consequences of anæmia may be expected.

The Treatment of Anæmia.

The use of pathology is to guide us in the prevention and cure of disease, and treatment, to be scientific, must rest upon pathology. We have endeavoured to work out the pathology of anæmia, and have shown that anæmia depends upon either defective hæmogenesis or excessive hæmolysis, or upon the two combined. The principles of the treatment of anæmia must, therefore, follow upon

these lines. It will be convenient to consider treatment under three heads—dietetic, hygienic, and medicinal.

In *defective hæmogenesis*, whether the anæmia is idiopathic or symptomatic, it is essential that the diet should be ample to supply the pabulum from which the corpuscles and their hæmoglobin are prepared. It must be remembered that the food of ordinary life, in sufficient quantities, contains all the elements, including iron, that are necessary for blood formation. We must be careful to see to the condition of the teeth that mastication may be properly performed. A good amount of meat is desirable—as much as the patient can comfortably and perfectly digest. A small amount of stimulants—especially red wines, such as burgundy, claret, or port; or malt liquors, such as stout or light bitter beer, is often of service. The hygiene consists in securing the patient as much light—especially direct sunshine—and fresh air as is possible. The bleaching effects of darkness are seen throughout organic life. Gentle exercise in the open air, or driving, when the strength permits, is of importance, but, in severe degrees of anæmia, absolute rest is imperative and of the greatest service. In less severe degrees of anæmia longer hours than usual in bed, and the recumbent position for some hours in the day time, to save the enfeebled heart, are important matters in practice. A morning tepid sponge-bath should be used. Suitable and sufficient, but not excessive or tight, clothing should be worn. Anæmic persons are usually sensitive to cold.

We come now to the *medicinal* treatment. The following are the remedies which increase the rapidity of, or perfect, blood formation:—Iron, phosphorus, potash, manganese, arsenic (?), hydrochloric acid (indirectly), and oxygen.

Iron is proved by every-day clinical experience to be of the greatest value in anæmia, though objectors are still found to its being absorbed and acting on the blood. They believe that its good effects are due to its local tonic action on the stomach and intestine. That it is, in some part, absorbed has been proved by Rabuteau and many other observers. Much that is taken is, undoubtedly, not absorbed, and passes into the intestine, where it is converted into the sulphide or tannate, which cause the black stools of patients taking iron, and it is probable that much that is absorbed into the portal vein returns to the intestine with the bile. In the stomach and intestine, if not given in these forms, it

becomes converted into a chloride or albuminate. It soon becomes incorporated with the red corpuscles, as it cannot be detected in the plasma elsewhere than in the portal system. I cannot refrain from quoting an interesting remark of Dr. Mitchell Bruce's:* "The action of iron on the blood is almost unique of its kind: First, because its specific action is exerted, not upon the plasma, but upon the red corpuscle, and on this alone, not on any other tissue or organ. Secondly, because the action appears to be nothing more than the combination of iron as one of the constituent elements of the corpuscles with the others. In the case of no other metal can we speak so definitely of its *modus operandi*." It being accepted, as all clinical experience teaches, that iron does improve the character of the blood, we must next examine into how it accomplishes its effect. Systematic examinations of the blood in chlorotics and other subjects of anæmia show that usually its immediate effect is to increase the number of corpuscles without a corresponding increase in the amount of hæmoglobin, so that the first result may be a lowering of the worth of the individual corpuscle. By degrees, however, the corpuscles become more deeply impregnated with hæmoglobin, and, ultimately, they reach their normal value. It would thus appear that the influence of iron is first to stimulate the production of new or young corpuscles, and only secondarily does it add to the amount of hæmoglobin in the corpuscles. The practical matter that concerns us most is the determination of the best manner of administering it. Probably all forms are useful, and, possibly, the form makes no difference if enough can be given. The bland and non-astringent forms are tolerated in larger doses and for a longer time than the astringent forms. It is, again, a question whether ferrous or ferric salts are the most useful as hæmatinics. This is, in a measure, answered in the preceding paragraph, and, speaking generally, my experience leads me to the conclusion that the ferrous salts are to be preferred.

A few words on the method of conducting an iron treatment will not be out of place. I would remark it is exceedingly rare for patients to be unable to take iron in some form when it is really needed. Next, the pill form is the most convenient in the usual way. It does not blacken the teeth, is tasteless, the dose is easily augmented without modification of the formula, and is proved by

* 'Materia Medica and Therapeutics,' 4th ed., p. 85.

experience to be thoroughly efficient. In cases where there are reasons for not giving it in pill, albuminate of iron, dialysed iron, Griffith's mixture, or one of the scaly preparations may be employed. Where there is much gastric disturbance this must be treated at the same time, but is no bar to the administration of iron. When the tongue is coated, flabby, and indented by the teeth, it is my practice to give an alkali and bitter mixture between meals, and iron in pills immediately after or with food. We may combine the iron with the citrate or bicarbonate of potash, and give the two together; but it is not, in this way, so convenient to regulate and augment the dose of iron. Dr. Lauder Brunton recommends the astringent preparations of iron when the tongue is pale and flabby. Where constipation co-exists, as it often does, there are several ways of dealing with it, dependent on its cause. Dietetic measures should always be employed; but, failing these, we may combine the iron with an aperient in the form of a mixture, as the sulphate of iron and sulphate of magnesium, or the iron may be combined in a pill with aloes and myrrh. My own plan is, however, usually to give a saline aperient in the morning on rising. One of the natural purgative waters may be employed, or a drachm (or thereabouts) each of sulphate of sodium and sulphate of magnesium (with or without a drachm of chloride of sodium) in half a tumblerful of warm water. My distinguished colleague and friend, Sir Andrew Clark,* has particularly emphasised the importance of aperients in the treatment of chlorosis, and states: "If, in the treatment of this malady, I were perforce limited to the employment of one sort of drug, the drug which I should choose for use would be an aperient." Nevertheless, he usually advises the employment of an acid or alkaline ferruginous cathartic mixture, or sulphate of iron in pill, and a saline aperient in the morning, as recommended above.

Fashion, no doubt, influences our choice of the particular preparation, and so one salt or combination of iron after an another is for a time the popular medical remedy. The teaching of Niemeyer and the pushing of advertising pillmongers have made Blaud's pill the general favourite at the present time, and it is, undoubtedly, one of the most valuable. I have used this preparation for the last twenty years with excellent results. The slight pharmacological knowledge I possessed told me that the amount

* 'Lancet,' vol. ii, 1887, p. 105.

of carbonate of potassium was in excess, and for years I have prescribed the pills in the following formula:—Ferri sulph. exsic. gr. ij; potas. carb. gr. j; glycerin. of tragacanth q.s. Blaud's pill being so much in use, it is interesting to learn what we can about it. A good deal has been written on the exact condition of the iron in these pills. Mr. Martindale has been good enough to supply me with some interesting information. "The following formula represents the decomposition in making Blaud's pills:—

$$K_2CO_3 + FeSO_4 = FeCO_3 + K_2SO_4.$$

By exposure in the moist condition $FeCO_3$ evolves CO_2, and absorbs oxygen, becoming converted into ferric oxide as ferric hydrate. Thus:

$$FeCO_3 - CO_2 = FeO,$$
$$2FeO + O + H_2O = Fe_2O_2(OH)_2."$$

Mr. Martindale suggests that, "instead of equal quantities of carbonate of potassium and sulphate of iron (crystals), about three of the former and five of the latter should be used, so as not to have an excess of the potash salts, which makes the pill become damp; or equal weights of dried sulphate of iron and carbonate of potassium nearly exactly neutralise each other. A 5-grain Blaud's pill, according to the 'Addendum to the British Pharmacopœia, 1890,' should contain about 1 grain of true carbonate of iron, as ferrous carbonate; but it is found that before the pills can be dried and coated quite one-half of this has been converted into ferric oxide."

Of course it may be maintained that there is a great difference between pharmaceutical elegance and practical results, and that the excess of alkali in Blaud's pill, as prescribed by Niemeyer (modified from Blaud's original formula), increases their efficiency, if it gives the dispenser more trouble. I have not found, however, that the diminished quantity of alkali in any way impairs the utility of the pill. Potash is the principal base of the red corpuscles, and potash is found in the urine in excess in some forms of anæmia, so that the blood may be poor in it. The latter, however, is the case when the anæmia is hæmolytic, and when iron is, as a rule, not indicated; whilst in anæmia due to defective hæmogenesis there is no loss of potassium. Moreover, the potassium is so abundantly supplied by the food, that the amount given in

the pill is a mere drop in the ocean. Two other pill preparations containing iron deserve mention. The pil. ferri carb., 'B.P.,' made with saccharated carbonate of iron and *syrup* instead of conf. rosæ, as suggested by Mr. Martindale, is an excellent one, which I have been using lately. A 5-grain pill will contain $1\frac{1}{2}$ grains of ferrous carbonate—three times as much as Blaud's pill—and is much more stable. Five grains of dried sulphate of iron made into a pill with syrup—a favourite of Dr. Ringer's—also contains three times as much iron as Blaud's pill.

In treating primary, especially chlorotic, anæmia, the best results appear to be obtained by pushing the iron in large doses, and it is on this account that the pill form is so convenient. We can begin with one pill three times a day, soon give two pills three times daily, and later, in some cases, give three pills for a dose. More than this is not usually necessary. No doubt much of this is squandered, not being absorbed from the digestive tract. Whether in these circumstances more is actually absorbed than when it is given in smaller doses, or whether it acts indirectly on the gastro-intestinal tract, as maintained by Buchheim, Kobert, and Reinl, is uncertain; but the fact that large doses act more promptly and efficiently is established by experience.

One more point of great importance which is often lost sight of. The iron treatment, in cases of chlorosis especially, should be persevered in for two or three months after the patient feels herself well, and often has a good colour. We have seen that the hæmoglobin remains deficient after the corpuscles have returned to about their normal number, and it is only slowly that the corpuscles arrive at their normal amount of hæmoglobin. Relapses in such cases are exceedingly common if the iron is prematurely discontinued. Laache, in a recent communication,* has emphasised this fact, and pointed out that Niemeyer's statement, that three boxes of 96 Blaud's pills suffice to cure the most obstinate chlorosis, is very misleading.

Phosphorus, in my experience, is of greatly less value than iron in chlorosis and other forms of defective hæmogenesis. It is, however, sometimes serviceable, as in cases recorded by Gowers† and others. When given it should be in pill made with ol. theo-

* Sep.-Abdr. aus d. 'Wiener klin. Wochenschr.,' 1890, No. 38.

† 'Practitioner,' July, 1878.

bromæ, and recently prepared.* It has been maintained by some recent writers that dilute hydrochloric acid is more useful than iron (Zander). It may do good by increasing the digestive power, and thus enable the patient to gain more iron from the food. Manganese appears of doubtful value. Arsenic has been proved to be useful in some cases of chlorosis, but iron is usually much more efficacious. Oxygen, administered by inhalations, has been used in some cases with, apparently, good results; but I have no personal experience of this method of treatment. We occasionally meet with cases of chlorosis which improve time after time with iron and other remedies, but which, ultimately, die of this disease.

We now pass on to the treatment of *hæmolytic anæmia*. Here theoretical considerations suggest that animal food should be sparingly given, since it has been pointed out in the first lecture that it is asserted that a diet rich in animal food increases active destruction of red corpuscles. Dr. Hunter† has suggested a trial of a purely milk diet, or of a farinaceous and milk diet; and in the case in which he tried these, though the disease was in an advanced stage, a certain amount of improvement took place—the blood pigment disappeared from the urine, and the amount of aromatic sulphates was diminished. I shall certainly give a purely milk and a farinaceous and milk diet a trial. Alcohol does not, as a rule, benefit such cases, and, if given, weak spirit and water is the best form. As long as patients with pernicious anæmia have the strength, they should get into the open air and sunshine, but should not tax their powers by exercise. They should adopt the recumbent position as much as possible, so as to conserve their powers, and put as little tax as possible on the fatty and weakened heart. We must remember, however, whilst combating the hæmolysis, not to neglect hæmogenesis, which is often struggling to maintain the balance. Sitting in the open air and sunshine in suitable weather and carriage exercise are, therefore, to be encouraged, the patient, of course, being suitably clothed.

We come now to the medical treatment of pernicious and other forms of hæmolytic anæmia. Amongst the remedies which diminish blood destruction—*anti-hæmolytics*—may be enumerated

* The 'Extra Pharmacopœia,' Martindale and Westcott, 6th ed., 1890, p. 295.

† 'Med. Soc. Trans.,' vol. xiii.

arsenic, quinine, mercury, phosphorus, beta-naphthol, iodoform, carbolic acid, sulpho-carbolates, and menthol.

Arsenic.—Amongst the great gains we have had in the treatment of anæmia in recent years, arsenic stands in the first position. We owe the suggestion of its employment to Dr. Byrom Bramwell,* who was the first to use it in pernicious anæmia, in 1877. Since then abundant testimony has confirmed its value, and its action, if not specific, is at least very marked where tolerated. Various modes of administration have been adopted, but none is more efficient than Fowler's solution, in which form usually the maximum quantity can be exhibited. The dose, beginning with quite a small one—2 to 4 or 5 minims—freely diluted, and given with or directly after food, can be gradually increased until largish ones are tolerated. I believe the larger doses—10 to 15 minims three times a day—to be the most capable of producing good results. In some cases of pernicious anæmia arsenic is not well borne, though I cannot remember to have had such a case under my own care. In such circumstances I should certainly try its administration subcutaneously. It is not always successful, even when tolerated. As regards its mode of action, we have no certain knowledge. It was originally tried on account of its undoubted, though somewhat capricious, efficacy in Hodgkin's disease, which led to the inference that it exercised some, although unknown, action on the blood-forming organs. It has been suggested that it acts locally on the mucous membrane of the stomach and intestine (Hunter)—I presume as a germicide, preventing the development of ptomaines. Its well-known action in malarial anæmia is probably due to its germicidal action on the plasmodium malariæ in the blood corpuscles. A new light has been thrown on its possible action by an interesting observation of Dr. Copeman's.

He writes† of pernicious anæmia: "When a drop of blood was removed from the finger and allowed to fall on a glass slide, then, when the edge of the drop had dried somewhat, a cover glass was gently placed upon it, crystals of hæmoglobin gradually formed in the film of blood in from 10 to 48 hours without any further preparation. The only exception to this was in the case of patients who had been treated with arsenic for some days, after which

* 'Edinburgh Med. Journ.,' November, 1877; and 'Med. Times and Gaz.,' 1877, vol. ii, p. 431.

† 'St. Thomas's Hospital Reports,' vol. xviii, p. 103.

crystals could not be obtained, although, if then the arsenic were discontinued for an equal length of time, they again put in an appearance."

It would appear from this important observation that arsenic exercises a direct influence upon the red corpuscles, lessening their vulnerability, which we have seen is one of the characteristics of anæmia, especially of hæmolytic origin. Arsenic has been shown to diminish the glycogenic function of the liver, and it is possible it may exercise some influence in restraining the hæmolytic function of this organ. In poisonous doses arsenic exercises a destructive influence on the red corpuscles, reducing the hæmoglobin and impairing its oxygenating powers. An interesting point in connection with the administration of arsenic is the fact that it sometimes produces bronzing or pigmentation of the skin. How this is brought about is a disputed point; but it is probable that it is by increasing the activity of the pigment-producing tissues. It is probable that the increase of colour of the urine in some cases of pernicious anæmia during the administration of arsenic is in some way connected with this action.

Quinine is sometimes useful in the pyrexial attacks of pernicious anæmia. Phosphorus has proved useful in one case recorded by Dr. Broadbent, but has failed to do good in others. The evidence regarding mercury is conflicting, but Keys has asserted it increases also the number of red corpuscles. Beta-naphthol has been suggested by Dr. Hunter, and this drug and the other disinfectants I have named perhaps deserve a trial, but I have no personal experience of their employment in anæmia. Too often in pernicious and other forms of hæmolytic anæmia, all treatment is incapable of preventing the final issue in death. Arsenic may arrest the disease for a time, and in a few cases effect a cure, but more often, as we have seen, its effects are more transitory than is generally believed. Indeed, the position to-day is but little altered since Addison said of it, "The disease presented in every instance the same general character, pursued a similar course, and, with scarcely a single exception, was followed after a variable period by the same fatal result."

I had intended to have dealt with transfusion in the treatment of anæmia, but my time is exhausted, and I must leave this undiscussed. This is less to be regretted, as my own experience on the subject is small, and distinctly unfavourable. The literature

is voluminous, and the matter a very complex one. Moreover, the subject has been very fully dealt with by M. Hayem, and quite recently by Dr. Hunter,* and the outcome of their experiments, and the general medical experience, is against the employment of transfusion, though cases of recovery from pernicious anæmia have been recorded after transfusion. Intraperitoneal and subcutaneous injections of blood have also been tried, especially on the Continent, but such treatment, even in the most skilful hands, has been shown by Cohnheim not to be free from great danger, fatal peritonitis having occurred in several cases after intraperitoneal injections.

But I must now bring my lecture to a close. I am deeply conscious of the imperfect manner in which I have accomplished my task. I feel that, in trying to cover so wide a field, I have dealt in a sketchy and imperfect manner with many parts of the subject.† Still, I cannot but believe that it is important that a survey of the whole subject of anæmia should be made, so as to fit the many and detailed observations of the numerous workers into one harmonious whole. The enormous amount of work that has been done in recent years and the complexity of the subject render the office a difficult one, requiring an abler exponent than myself.

In conclusion, I desire to express to you, Sir, and the Council of the Society, my sense of the honour conferred upon me in inviting me to deliver these Lettsomian Lectures, and also to thank the Fellows and gentlemen who have patiently listened to them.

* Arris and Gale Lectures, 'British Medical Journal,' 1889, vol. ii.

† I regret exceedingly that a very able and important paper by Dr. Edington, "Report on the Morphology and Development of the Blood," 'British Medical Journal,' May 31st, 1890, has entirely escaped my observation until too late to deal with. It should be consulted by the reader.

January 12th and 26th, 1891.

ON CHRONIC DISEASE OF THE UTERINE APPENDAGES; WITH SHORT NOTES OF THIRTY CONSECUTIVE CASES TREATED BY ABDOMINAL SECTION.

By William Duncan, M.D., F.R.C.S. Eng.

Before entering on the systematic consideration of the various diseases, I propose to relate briefly the notes of one or two cases out of the thirty on which I performed abdominal section, seven of these being in private and twenty-three in hospital practice. For convenience of reference I have arranged all the cases in a tabular form.

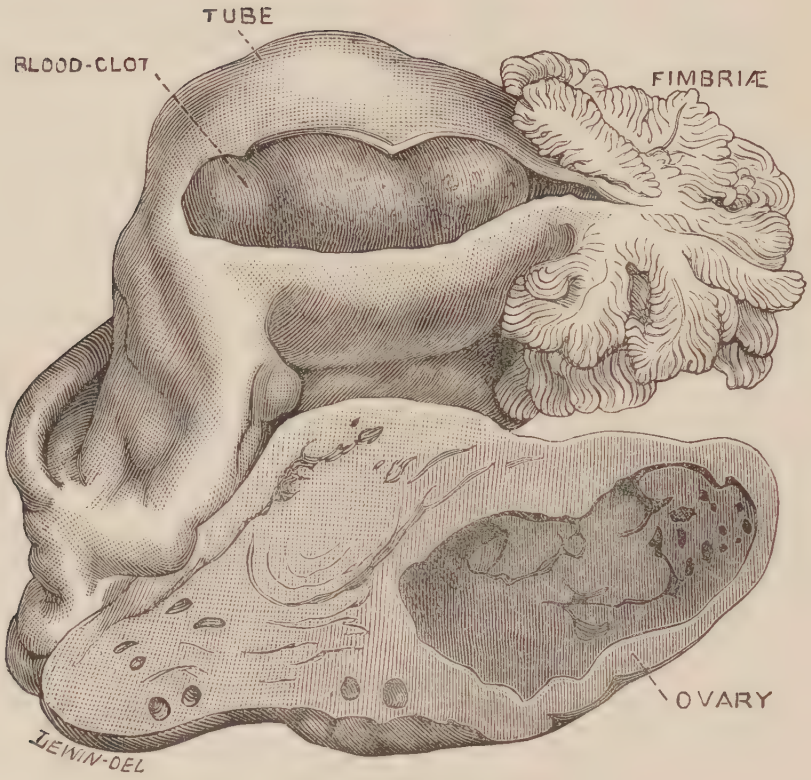
Case 6. *Small Suppurating and Adherent Dermoid Tumour of Right Ovary.*—Mrs. S——, aged 26 (an Australian lady), was sent to me by Dr. Cory, of Guildford, for chronic pelvic pain and dysmenorrhœa. The patient is a tall, thin, and delicate-looking woman. The catamenia began at 16, were regular and normal until four years previously, when she began to suffer pelvic pain, especially on the right side, and radiating through to the back; this pain has decidedly increased since her marriage two years ago; she has not been pregnant, and latterly there has been much dyspareunia. On examination nothing is felt abnormal in the lower abdomen. *Per vaginam* the uterus is not enlarged, but is pushed somewhat to the left side by a rounded, tender, somewhat elastic, and fixed tumour, about the size of an orange, in the right posterior quarter of the pelvis. This was diagnosed as an enlarged ovary, and the patient was kept in bed for two months. Hot iodine douches were used night and morning. Blisters were applied over the right ovarian region, and small doses of corrosive sublimate with iodide of potassium given thrice daily. No improvement resulting, abdominal section was performed, when the tumour proved to be a suppurating dermoid, with universal adhesions, so that its removal was exceedingly difficult. During the operation some of the purulent contents escaped into the pelvic cavity, and there was oozing from the torn adhesions. The pelvic cavity was flushed with a hot 5 per cent. boracic solution and a glass drainage tube inserted. The tube was removed in thirty-six hours. The patient progressed satisfactorily until a week after the operation, when a small hæmatocele occurred where the tumour had been. This made the patient's recovery tedious, but I saw her a year after the operation, when she expressed herself as feeling perfectly well in every respect, and she is putting on flesh. At the time of the operation the left tube and ovary had a few adhesions binding them down, but otherwise they appeared healthy; so the adhesions were divided, and the appendage left intact.

Case 24. *Pelvic Peritonitis; Right Hydro-salpinx, Left Ovary and Tube Matted Together; Cure.*—Mrs. H——, aged 27, married three years; has never been pregnant. Catamenia commenced at 15, always more or less painful, worse since marriage. In 1888 she was operated on in Australia for the cure of the dysmenorrhœa, which she was told was due to her womb being bent. She was ill after the operation from "inflammation." On coming to England in 1889 she consulted me. I found "uterus normal in position, but its mobility is impaired. In the left posterior quarter of the pelvis the ovary and tube feel matted together and enlarged; in the right quarter is a cystic swelling which feels like a dilated tube." On December 7th, 1889, I performed abdominal section, and found the left appendages universally adherent, with much thickening of the tube from chronic salpingitis. The right tube was dilated into a thin-walled cyst, and contained 4½ ounces of serous fluid; there was rather free oozing from the torn adhesions, but it was soon controlled by sponges. The patient's subsequent history is very interesting. Two hours after the operation sickness commenced, and continued, despite all treatment, with great frequency for six days, during which time the patient vomited 103 times. Thirty-six hours after the operation the retching was so severe that the wound was partly torn open and secondary hæmorrhage ensued. Harelip pins were used to give greater support. From the third to the sixth day the vomit was black. On the seventh day an enema of 15 ounces of olive oil was given, as well as a dose of white mixture every two hours by the mouth. These acted freely, and from that date all sickness disappeared. The patient was up on a couch on the nineteenth day. Since then she has been to Australia and back, and now (January, 1891) she is perfectly well, and enjoys violent exercise on horseback.

The next two cases are very interesting, inasmuch as they presented symptoms and physical signs of tubal gestation. Both are young married ladies, living close to one another in Hertfordshire, and were under observation and treatment at the same time.

Case 26. *Left Hæmato-salpinx with Apoplexy of Ovary.*—Mrs. E.——, aged 20, married six months. She was regular for three periods after marriage; she then saw nothing for seven weeks, and had morning sickness, with pains in the breasts and lower part of the back. At the end of the seven weeks she was seized with severe pains in the left side of the abdomen and in the back, accompanied by hæmorrhage. A clot was passed, but this was not shown to her medical attendant, Dr. Ambler, of Hemel Hempstead. Under treatment and rest the loss diminished for a time, but the pain still remained. The hæmorrhage again increased, so Dr. Ambler asked me to see her. I found on examination the uterus somewhat enlarged, the os uteri admitting the finger tip. In the region of the left Fallopian tube, and extending somewhat behind the uterus, was a tense, elongated cystic swelling, apparently about the size of a sausage. The sound was not passed. I diagnosed a tubal pregnancy, and, in view of its possible rupture at any moment, recommended that abdominal section should be done without delay. I operated next morning, and without much difficulty removed the specimen I now hand round. The ovary is much enlarged, and on section is seen to be filled with blood; the Fallopian tube is distended to the size of a banana, and it also is full of blood, partly fluid, partly clotted. (Fig. 1.) The patient made an uninterrupted recovery, and is now (January, 1891) in perfect health.

Fig. 1.

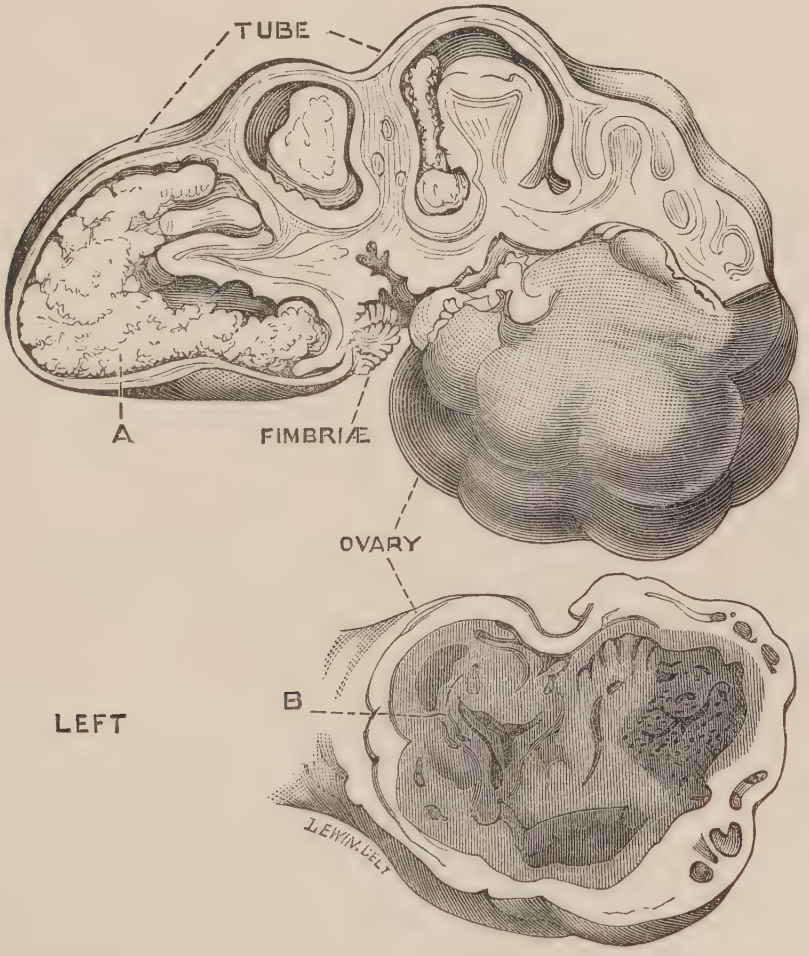


Case 26.—Hæmato-salpinx and apoplexy of ovary.—Tube distended with blood and blood-clot. Ovary enlarged. Hæmorrhage into (probably) a greatly distended Graafian follicle.

Case 27. *Double Tubercular Pyo-salpinx, with Tubercular Disease of both Ovaries and Apoplexy of Left Ovary.*—Mrs. M——, aged 22, married two years and a half; never pregnant. The family history is good, except that one maternal uncle died of phthisis. Previous history good. Catamenia began at 15, regular every four weeks, and lasting one week; but ever since the onset of the periods there has been pain in the left ovarian region. Since marriage the dysmenorrhœa has been much worse, and clots have been passed. On June 9th, 1890, she consulted her medical attendant, Dr. Russell Steele, of Hemel Hempstead, as the pain was so severe. She then had amenorrhœa for six weeks with some morning sickness, and thought she was getting stouter. On making a vaginal examination, Dr. Steele felt an irregularly shaped mass, very tender to the touch, to the left side of, and slightly behind, the uterus. Bimanually little could be made out, owing to thickness of the abdominal parietes. Rest in bed, with sedatives, was ordered. Fourteen days later Dr. Steele was suddenly summoned to see the patient, and, on arrival, was informed that she had been to the water-closet, and was there suddenly seized with the most excruciating pain in the lower abdomen. "Something" passed from her into the pan of the closet during the pain, but, unfortunately, it was not kept. Dr. Steele found the patient faint and collapsed, but, after administering stimulants and a hypodermic injection of ether, she quickly rallied. When I saw the patient, in consultation with Dr. Steele,

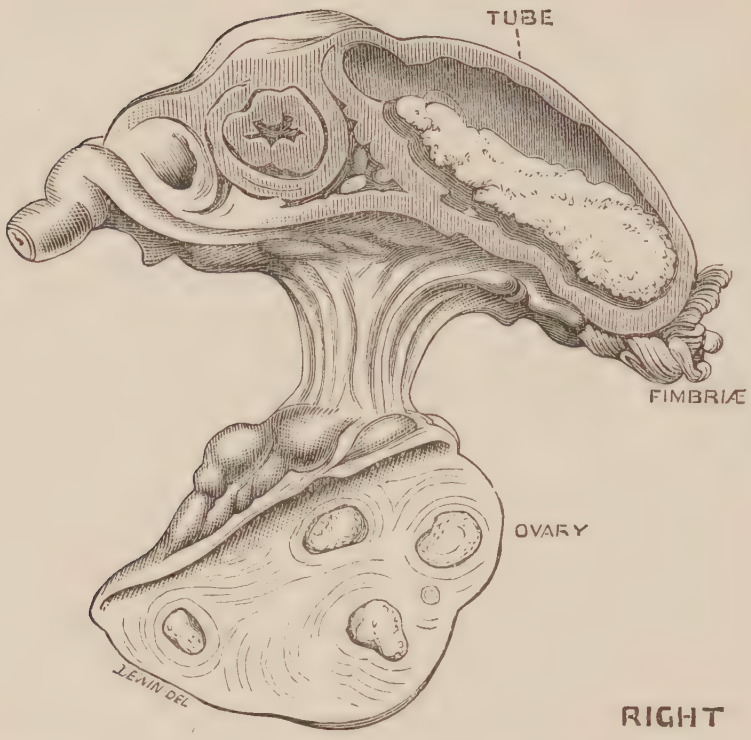
the condition was just as described above, and I concurred in his opinion that most likely it was a ruptured extra-uterine gestation ; but, as I thought it had ruptured between the layers of the broad ligament, I recommended strict confinement to bed and watching the progress. As a week later she seemed getting worse, pain more severe, pulse feebler, I performed abdominal section, with the help of Dr. Steele and Mr. Battle. On opening the abdomen the peritoneum was seen to be studded with rice-like bodies. The left broad ligament was apparently distended by a tense, bluish, elastic swelling, but, after tearing down many adhesions (a process which took more than an hour), the swelling proved to be a distended Fallopian tube and enlarged ovary matted together. These were removed, after transfixing the broad ligament, in the usual way. The appendages on the right side were removed in the same manner after breaking down many adhesions. A drainage tube was inserted, and the wound treated in the ordinary way. On examining the parts removed (see Figs. 2 and 3) the left Fallopian tube was distended with thick, inspissated pus to the size of a sausage. The left ovary was as large as a small Tangerine orange,

FIG. 2.



CASE 27.—Left ovary and tube.—*Ovary* enlarged, adherent to tube, and full of blood (B). *Tube* distended with inspissated tubercular pus (A).

FIG. 3.



CASE 27.—Right ovary and tube.—*Ovary* twice normal size, containing deposits of cheesy pus. *Tube* distended with a like deposit.

cystic, and with hæmorrhagic extravasation into it. The right tube was about as thick as a man's thumb, and contained the same kind of cheesy pus as the left. The right ovary was double the normal size, and was studded with collections of pus the size of peas. The patient made a rapid and complete recovery, and is now putting on flesh and feeling well.

The next case is instructive and interesting, as the pedicle ligature slipped soon after completion of the operation. Hæmorrhage (which was not diagnosed) took place, and death followed two days later.

CASE 29.—Mrs. J—— was admitted to hospital complaining of dysmenorrhœa, pains in the back and left side, leucorrhœa, and general ill-health. Married thirteen years; has had three children. On admission the uterus was retroverted, and not easily replaced; there were thickening and tenderness in the region of both broad ligaments. She was treated for several weeks with hot douches, glycerine tampons, blisters, and aperients. When re-admitted, two months later, the patient was no better, so abdominal section was performed. The ovaries and tubes were adherent in the pelvis; they were removed and the pedicles were treated in the ordinary way. Half an hour after the operation the patient was very collapsed; pulse feeble, regular (70); unconscious; no distension of abdomen. Temperature 96·4°. Hot blankets were applied, ether injected,

and, as she seemed to rally, I was not sent for. An hour and a half after the operation reaction had well set in; lips good colour; pulse strong and full (80). A morphia suppository of a $\frac{1}{4}$ of a grain was given. I saw the patient three hours and a half after operation. She appeared quite satisfactory, no signs whatever to indicate internal hæmorrhage. The next day she seemed comfortable; the temperature was 100°. Slight tenderness of abdomen; no distension; resonant all over; no sickness. Towards evening the temperature went up to 102·4°, and next day (the second after the operation) the patient was evidently worse; pulse rapid, weak (136); abdominal pain, tenderness, and distension. Diagnosing peritonitis, I re-opened the abdomen to wash out the cavity, and was horrified to find it full of blood; the broad ligament was rapidly seized, the blood and clots cleared out, pedicle securely retied, abdominal cavity irrigated, a drainage tube inserted, and the wound quickly reclosed. In spite of all efforts at stimulation the patient never rallied, but died six hours after.

Let us now consider *seriatim* the different diseases of the uterine appendages included under the title of this paper. It is obviously unnecessary to touch upon large tumours which rise up out of the pelvic cavity, as nowadays almost everyone agrees that their proper treatment is by abdominal section. Such, however, is unfortunately not the case with diseases which give rise either to small pelvic tumours or to none at all, but which often entail intense suffering, which are sometimes overlooked (either accidentally or designedly), and about the treatment of which there is (unluckily for the sufferer) a great diversity of opinion.

Chronic salpingitis is in the majority of cases due to extension of catarrhal or purulent endometritis; sometimes, however, it is doubtless secondary to a pelvic peritonitis. Each Fallopian tube consists, as is well known, from without inwards, of peritoneum, circular and longitudinal muscular fibres, with a mucous membrane, which is lined by a layer of columnar ciliated epithelium, and thrown into numerous longitudinal folds or plicæ. Glands have been described as present, but there is a complete absence of them. Probably the folded appearance of the mucous membrane led to the error. The changes which take place in the tubes when subject to chronic inflammation are well shown in this drawing (Fig. 4), taken from the beautiful plates of Wyder, of Berlin. The mucous and muscular folds are hypertrophied. The folds of the mucous membrane do not show the normal complicated appearance. Single papillary elevations are seen separated from one another by gland-like invaginations of the mucous membrane, some shallow, some deep. The papillæ are completely bared of epithe-

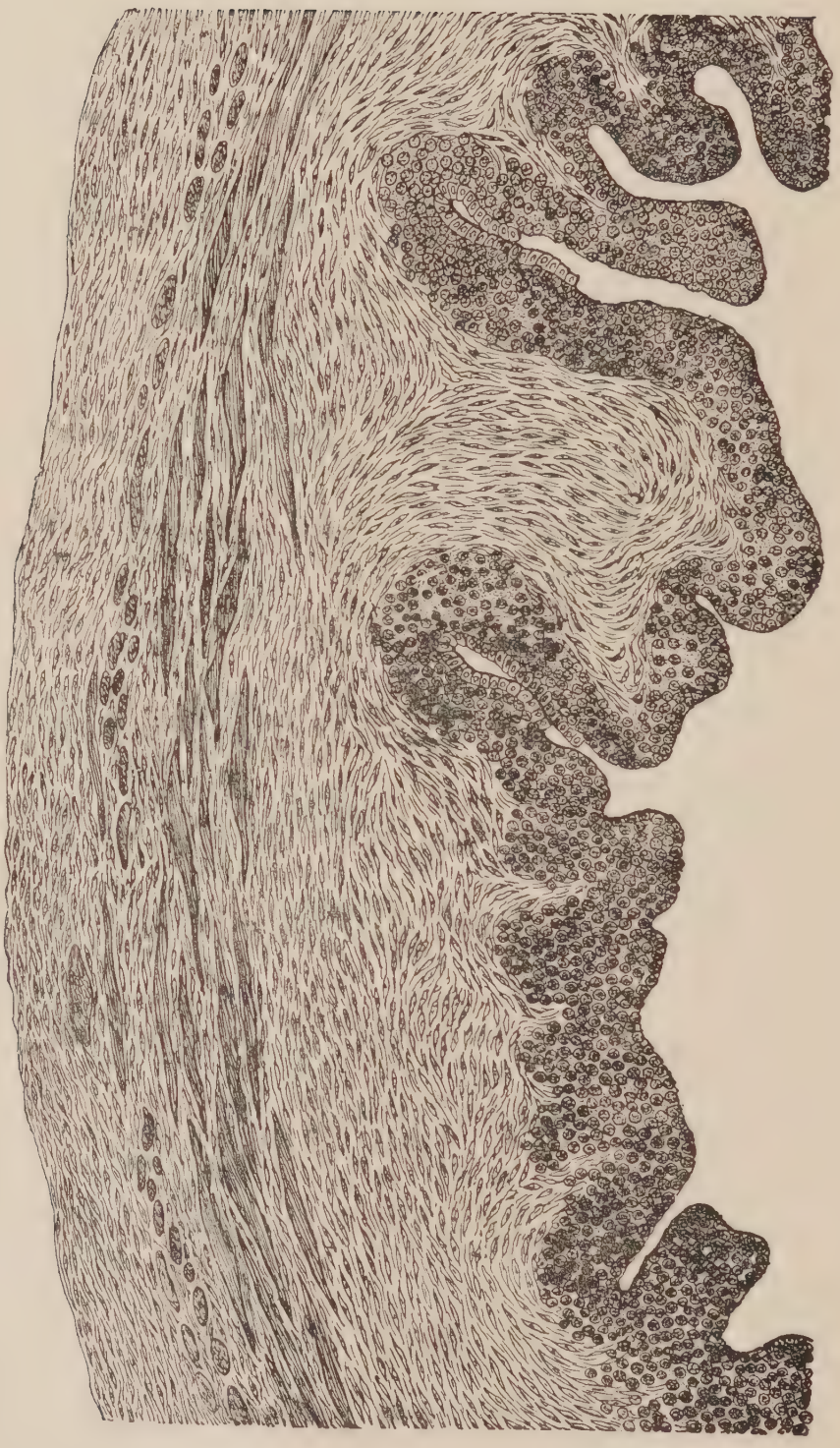


FIG. 4.
A

B

A. Section of Fallopian tube in case of pyo-salpinx (Wyder).—Mucous and muscular coats hypertrophied; ciliated epithelium lost, except in depressions between papillæ; superficial layer of mucosa replaced by round cell granulation tissue; deep layer made of spindle-celled tissue. B. Section of Fallopian tube in case of hæmato-salpinx (Wyder). Loss of epithelium, except in the depressions between the papillæ; round-celled tissue not so well marked as in A. Rich network of capillaries engorged with blood, and extending close up to the surface of the mucosa. Extravasations of blood at different parts.

Thirty consecutive Cases of Abdominal Section for Chronic

No.	Name.	Age.	State.	Symptoms.	Duration.	Physical signs.
1	A. L. ...	20	Single	Great dysmenorrhœa; painful defæcation	7 years ...	Elastic tumour, size of orange, left side of pelvis
2	Mrs. H. ...	30	Marr.	Constant backache; pelvic pain	6 ,, ...	Two elastic, tender, fixed tumours in pelvis
3	Mrs. L. ...	32	,,	Menorrhagia; constant pelvic pain	4 ,, ...	Retroflexed fixed uterus; distended left tube
4	Mrs. W....	25	,,	Left ovarian pain; constant backache	5 ,, ...	Uterus, ovaries, tubes fixed posteriorly
5	F. B. ...	24	Single	Left ovarian pain; great dysmenorrhœa	6 ,, ...	Elastic tumour, size of orange, on left side
6	Mrs. S. ...	25	Marr.	Chronic severe pelvic pain ...	4 ,, ..	Elastic tumour to right of uterus, size of orange
7	Mrs. P. ...	21	,,	Dysmenorrhœa; dyspareunia; menorrhagia	18 months	Sausage-shaped tumour on right; left tube thickened
8	J. T. ...	26	Single	Menorrhagia; dysmenorrhœa; leucorrhœa	5 years ...	Uterus retroverted; both ovaries large; prolapsed
9	Mrs. H. ...	27	Marr.	Constant pelvic pain; dyspareunia	17 months	Cystic swelling on right; left appendage enlarged
10	Mrs. B. ...	34	Widow	Menorrhagia; right pelvic pain	18 ,,	Uterus retroverted; prolapsed, thickened, and fixed appendages
11	A. J. ...	22	Single	Severe pelvic pain	4 years ...	Tumour on left; right ovary atrophied
12	Mrs. A. ...	35	Marr.	Left pelvic pain; yellow discharge	2 ,, ...	Uterus retroverted; tender swelling to left
13	Mrs. V. ...	33	,,	Severe dysmenorrhœa and menorrhagia	10 ,, ...	Ditto ... ...
14	Mrs. D. ...	30	,,	Chronic pelvic pain; offensive yellow discharge	7 ,, ...	Elastic tumour, big as fist, on left side
15	Mrs. A. E.	22	,,	Menorrhagia; severe dysmenorrhœa	1 year ...	Irregular swelling on right ... ...
16	Miss F. ...	22	Single	Intense pelvic pain and dysmenorrhœa	2 years ...	Both appendages prolapsed, enlarged, and fixed
17	Mrs. M. ...	37	Marr.	Pelvic pain; dysmenorrhœa; leucorrhœa.	1 year ...	Round tumour, size of orange, anteriorly
18	C. R. ...	26	Single	Constant left pelvic pain ...	19 months	Prolapsed; enlarged ovaries ...
19	Mrs. M. P.	44	Marr.	Severe pelvic pain; leucorrhœa	5 years ...	Uterus retroflexed; appendages prolapsed; enlarged
20	R. B. ...	31	Single	Menorrhagia; dysmenorrhœa; leucorrhœa	3 ,, ...	Enlarged, prolapsed ovaries... ...
21	A. S. ...	23	Single primip.	Constant pelvic pain; yellow discharge	2 ,, ...	Uterus retroflexed; fixed; appendages fixed
22	Mrs. S. ...	55	Marr.	Left pelvic pain; dysmenorrhœa	15 months	Uterus retroflexed; fixed; appendages enlarged; prolapsed
23	E. B. ...	20	Single	Chronic pelvic pain; menorrhagia	4 years ...	Right ovary atrophied; tender; general pelvic tenderness
24	Mrs. H. ...	25	Marr.	Left pelvic pain; constant discharge	4 ,, ...	Ovaries and tubes fixed in pelvis ...
25	Mrs. W....	27	,,	Menorrhagia; dysmenorrhœa	1 year ...	Uterus fixed; broad ligaments thickened
26	Mrs. E. ...	20	,,	Menorrhagia; severe backache	1 month	Tense swelling to left of uterus ...
27	Mrs. M. ...	22	,,	Severe pelvic pain; collapse	5 weeks...	Tumour filling left side of pelvis ...
28	Mrs. H. ...	20	,,	Menorrhagia; dysmenorrhœa	13 months	Ovaries and tubes enlarged; fixed in pelvis
29	Mrs. J. ...	31	,,	Dysmenorrhœa; leucorrhœa	2 years ...	Uterus retroverted; broad ligaments thickened
30	Mrs. S. ...	37	,,	Pelvic pain; menorrhagia; dysmenorrhœa.	6 months	Cystic swelling

* Case 21 was operated on twice: first time, the appendages were so firmly adherent and the abdominal parietes retarded convalescence. Four months later patient returned, begging that something more should be done to relieve compelled to desist. Patient went out a month later *in statu quo*.

Disease of the Uterine Appendages (one fatal from Hæmorrhage).

Where operated on.	Parts removed.	Result.	Remarks.
Waterloo-road Hospital	Left hydro-salpinx; both ovaries enlarged; cystic	Recovered	Perfect health two years later.
Middlesex Hospital	Double hydro-salpinx; both ovaries cystic	,,	Ditto.
Private ...	Left pyo-salpinx; right salpingitis; ovaries cystic	,,	Universal adhesions; operation very difficult; ultimate result excellent.
Middlesex Hospital	Left salpingitis and ovaritis	,,	Right appendage not removed.
Ditto	Left ovarian cyst; left salpingitis ...	,,	Right appendage separated from adhesions and left.
Private ...	Suppurating dermoid of right ovary ...	,,	Tumour universally adherent: ultimate result perfect.
Private ...	Right hydro-salpinx; left ovaritis and salpingitis	,,	Ditto.
Middlesex Hospital	Both ovaries large; left pyo-salpinx; right hæmato-salpinx	,,	Dense adhesions; perfect health resulted.
Private ...	Right hydro-salpinx; left salpingitis and ovaritis	,,	Universal adhesions; present health perfect.
Middlesex Hospital	Both ovaries atrophied; tubes much thickened	,,	Appendages matted together; suppurating parotid bubo; present health moderate.
Ditto	Ditto	,,	Suppuration in abdominal wall; retarded convalescence.
Ditto	Left appendage matted; much diseased	,,	Ditto.
Ditto	Large fibro-cystic left ovary; left hydro-salpinx	,,	Ultimate result good.
Ditto	Left ovary and tube matted together; right ditto	,,	Ditto.
Waterloo-road Hospital	Right hydro-salpinx; left salpingitis and ovaritis	,,	Ditto.
Private ...	Ovaries and tubes enlarged and matted together	,,	Cause: pelvic peritonitis following chill; present health excellent.
Middlesex Hospital	Ovarian tumour full of blood; twisted pedicle	,,	Three tight twists held fast by adhesions.
Ditto	Fibro-cystic ovaries, size of horse-chestnuts	,,	Ultimate recovery good.
Ditto	Both tubes distended; adherent to ovaries	,,	Ditto.
Ditto	Ovaries large, fibrous, cystic; tubes normal	,,	Present health excellent.
Ditto	Nothing removed	,,	See note below.*
Ditto	Both ovaries cirrhotic; both tubes dilated	,,	Dense universal adhesions; fixed uterus replaced; ultimate result good.
Ditto	Both ovaries ill-developed; very small...	,,	Uterus ill-developed; present health satisfactory.
Ditto	Ovaries and tubes matted together ...	,,	Present health excellent; constipation only trouble.
Waterloo-road Hospital	Left ovary apoplectic; double hydro-salpinx	,,	Present health good.
Private ...	Large apoplectic ovary; left hæmato-salpinx	,,	All signs and symptoms of tubal gestation; now quite well.
Private ...	Large apoplectic left ovary; double pyo-salpinx	,,	
Waterloo-road Hospital	Chronic ovaritis and salpingitis; appendages adherent	,,	Universal adhesions; operation very difficult.
Middlesex Hospital	Ditto	Death	Right pedicle ligature slipped; extensive hæmorrhage.
Waterloo-road Hospital	Double hæmato-salpinx; right ovary apoplectic; left ovary cirrhotic	Recovered	Present health good.

several inches thick, that it seemed impossible to remove the disease. Troublesome suppuration in abdominal wall her pain. On again opening the abdomen, the omentum and intestines were so matted to the parietes that I was

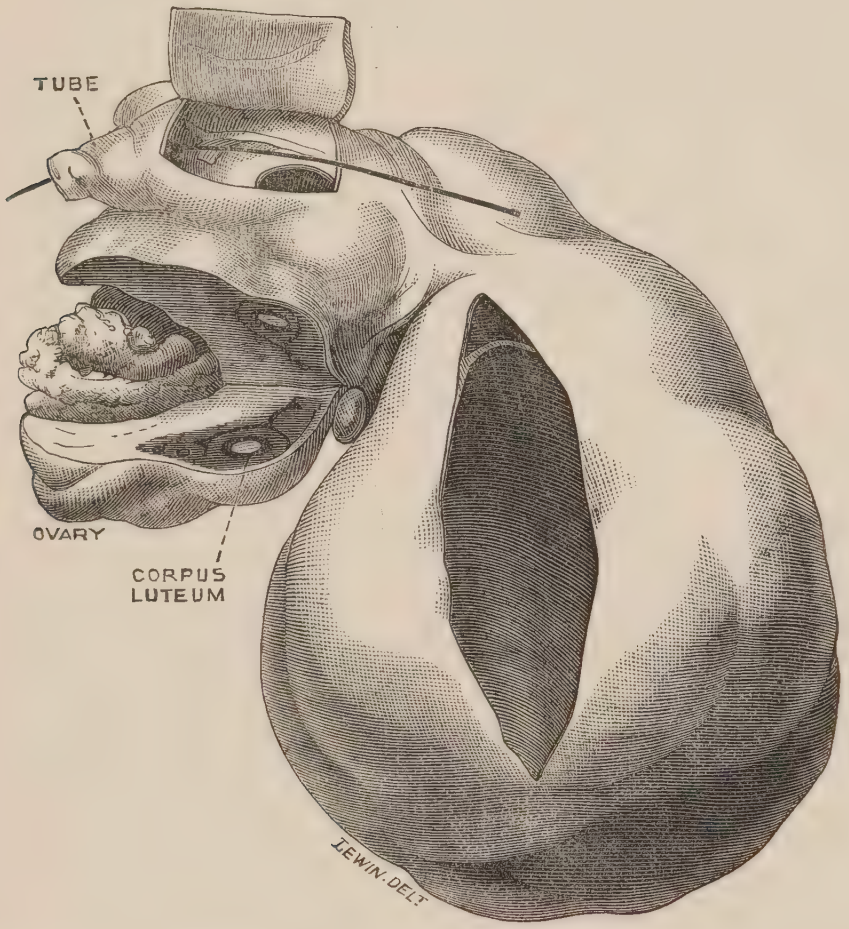
lium, but it may still be seen in the recesses between the folds. On the surface of the mucous membrane is a zone of closely packed round cells like granulation tissue, whilst beneath this the deeper portion of the mucous membrane is specially characterised by the presence of a large number of spindle cells. Thick-walled vessels are present in great numbers in the wall of the tube. I believe a salpingitis may exist like this for an indefinite period, the fluid exuded gradually making its way through the patent uterine end into the uterine cavity, but what happens far more often is this: the inflammation extends to the fimbriated end; a little of the fluid escapes into the peritoneal cavity and sets up a pelvic peritonitis of varying degrees of severity, according to the virulence of the exudation; then the ostium gets closed up by adhesions, and the tube becomes transformed into a shut sac by the uterine end also becoming closed. The further changes in the tube depend on the character of the primary inflammation. If it be catarrhal or due to extension of a peritonitis, then serous fluid collects in the tube, the coats of which get gradually thinned out, and finally we have a hydro-salpinx with a very thin transparent cyst wall. When the inflammation has been purulent (as in extension upwards of a gonorrhœa), then the same changes take place, with the important difference that the cyst contents consist of pus, forming a pyo-salpinx. In either case the capillaries of the diseased mucous membrane become gorged with blood, then some of them rupture, and blood gets extravasated into the tube, forming a hæmato-salpinx. This condition is shown in some of the specimens on the table, and the microscopical appearances are well illustrated in this drawing of Wyder (Fig. 4).

I said just now that as a result of salpingitis and pelvic peritonitis both the uterine and fimbriated ends of the tube become closed, and then dilatation takes place; this, however, does not invariably happen, for it is a positive fact that in some instances the uterine end of the tube remains patent, and that at certain times the distended tube discharges some of its contents through the uterus and vagina. This interesting condition is known as intermitting hydro- or pyo-salpinx, and may go on for an indefinite time unless cured by abdominal section. To explain the phenomena we have only to remember that dilated tubes sometimes undergo axial rotation in precisely the same manner as ovarian tumours; this rotation occludes the uterine end of the tube, which,

when further dilated, partly unwinds itself, and thus allows the escape of some of its contents.

This specimen (Fig. 5), which I removed quite recently from a patient in the Middlesex Hospital, is such a typical example of in-

FIG. 5.



Intermitting hydro-salpinx: Left Fallopian tube and ovary.—*Tube* greatly distended and coats thinned; abdominal ostium closed; uterine end (through which bristle is passed) patent. *Ovary* enlarged, adherent to tube. It contains a corpus luteum and a blood-clot.

termitting hydro-salpinx, and, as it cannot fail to convince the most sceptical, I will briefly relate the case.

M. A——, aged 36, was married at 20, and had one child a year later. About a fortnight after her confinement she got up, and two or three weeks later took cold, and was seized with great pain in the left side of the lower abdomen, for which she had to lay up three weeks. Nine months afterwards she began to have a white discharge between the periods, and this continued. In March

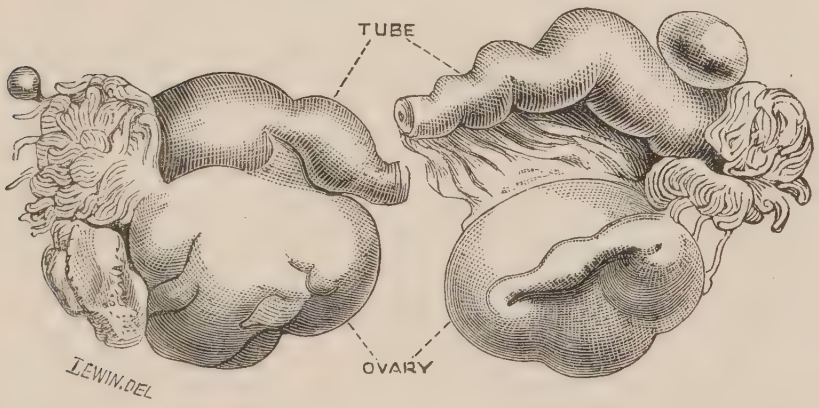
1888, she began to have a profuse watery discharge from the vagina at intervals on walking any distance, and she also had much pain in the back. This intermittent watery discharge continued until her admission to Prudhoe Ward on April 10th, 1889. On examination, my note was: "Left ovary and tube enlarged to the size of a small orange, and displaced below and behind the uterus. The right tube can be felt thickened and displaced downwards. The uterus is normal." I ordered hot vaginal douches night and morning, followed by glycerine tampons; also a mixture of quinine, iron, and sulphate of magnesia. Nine days after admission, a watery discharge commenced, and went on for a week, at the end of which time the swelling in the left posterior quarter of the pelvis was found to be much diminished in size; three weeks later she was discharged convalescent. On October 20th, 1890 (seventeen months after), she came again complaining of much pain in the back, left side of the abdomen, and left leg, with profuse gushes of watery fluid. On examination I then found a large cystic swelling filling up the left side of the pelvis. When the patient was admitted to the hospital three days later, the swelling was considerably reduced in size, and she said that she had had a large gush the previous day. On performing abdominal section I removed the hydro-salpinx now shown. There were no adhesions. I tied the tube close up to the uterus, and on cutting it across the contained fluid began to flow out, showing that the tube was patent right up to the uterus, and doubtless the uterine portion was so likewise. The right tube and ovary were found to be diseased, and were therefore removed. The patient's convalescence was retarded by some troublesome suppuration in the abdominal wall, which latter was thickly loaded with fat. Another condition, which has been erroneously thought to be the explanation of these cases of intermitting hydro- or pyo-salpinx, is that in which a dilated tube becomes adherent to the bottom of Douglas's pouch, and thus discharges its contents into the vagina, rectum, or bladder.

A most interesting case of this kind I saw some years ago in consultation with my brother, Dr. P. T. Duncan. The patient, a widow, was seized periodically with attacks of pelvic inflammation, followed by a profuse discharge of pus *per vaginam*. On examining the lower abdomen, I found on either side, rising up above Poupart's ligament, a tense elastic swelling, that on the right

being mobile, but that on the left was fixed, and it was noted that the left tumour diminished in size coincidently with the discharge of pus. Little could be made out on vaginal examination, owing to the pelvic inflammation. I diagnosed double pyo-salpinx, the left tube being adherent and forming a fistulous communication with the vagina. She refused to run the risk of operation, and I do not know what has become of her.

Passing on now to diseases of the ovary, the first and most common is *chronic ovaritis*. This may either be secondary to the acute form, or it may be due to extension of inflammation from the uterus and tube, or as a result of peritonitis and peri-ovaritis. The last is a by no means uncommon cause; the ovary gets bound down by adhesions, which after a time contract and produce a cirrhotic condition (Fig. 6). In other cases the ovary is enlarged

FIG. 6.



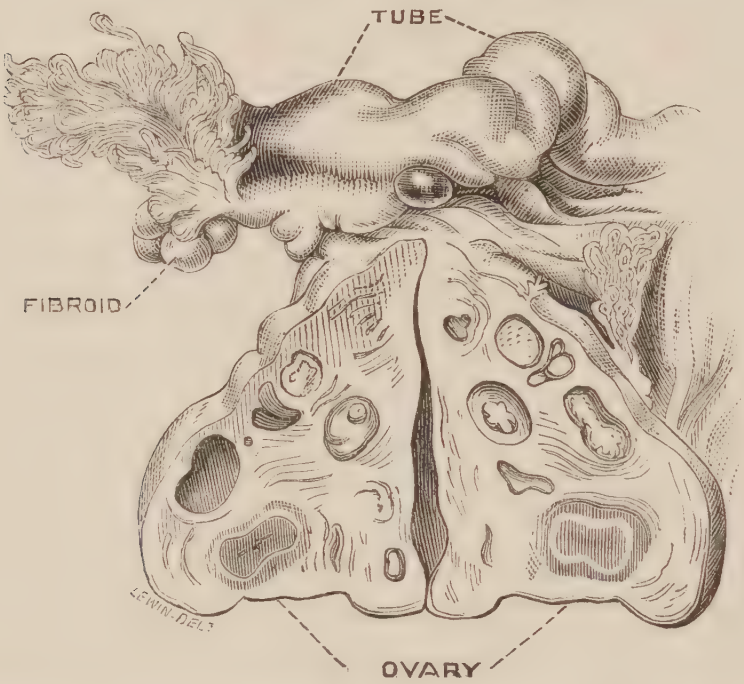
Ovaries and tubes matted together by adhesions.—*Ovaries* cirrhotic; coats of tubes thickened from chronic salpingitis.

but as a result of the chronic inflammation there is an increased development of fibrous tissues in the stroma (Fig. 7), with the formation of small cysts. In either case it is obvious that ovulation can only proceed with great difficulty, and the follicles as they enlarge may be unable to reach the cortex.

Congenitally ill-developed ovaries are not common; when they do occur the uterus is, in my experience, likewise ill-developed. The subjects of this condition may be fine handsome women, but they suffer from dysmenorrhœa, and if married are sterile. This drawing (Fig. 8) shows the condition well. I removed the specimen

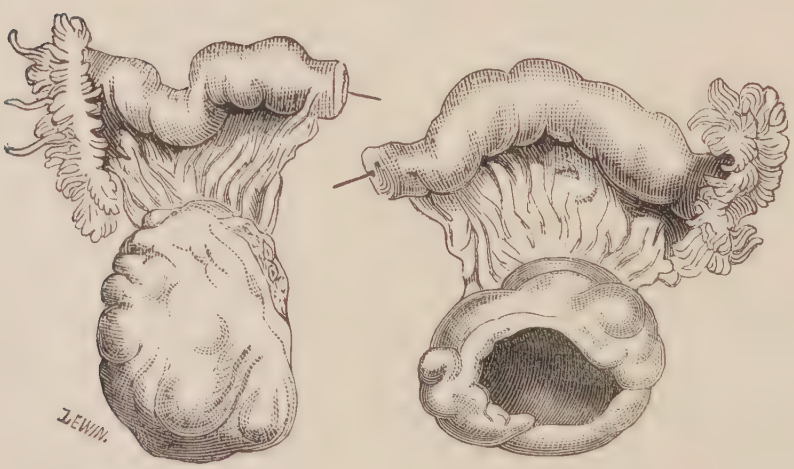
five years ago from a young woman aged 21, who consulted me for intense dysmenorrhœa. She began to menstruate at 17, always lost only very little for two days, but she described the pain she

Fig. 7.



Chronic ovaritis and salpingitis.—Tube thickened; ovary hypertrophied, fibrous, and full of cysts in its interior.

Fig. 8.



Congenitally ill-developed and cystic ovaries.

suffered as "fearful agony." It began a week before the period, and reached its acme two days after the period was over. She

was a tall, fairly healthy looking, dark girl, apparently not at all neurotic; she lived in the country, and took plenty of exercise in the intervals of the attacks. She has never had any illness beyond the diseases of childhood. On examination (under nitrous oxide) I found the uterus small and acutely anteflexed; the sound entered 2 inches; nothing else abnormal detected. The ovaries or tubes could not be felt. I recommended that the uterine canal should be dilated, and this I did up to No. 12, Hegar. Subsequently to this the following two periods were less painful, but then all the old trouble returned. After fully explaining the nature of the operation to the parents I performed abdominal section, and removed a pair of small ovaries with the tubes. The patient recovered without a single bad symptom, and she is now in perfect health.

Yet another case. A few weeks back a lady from Wales was brought to me by her brother-in-law (a medical man). She is 31 years of age, has been married ten years, and has never been pregnant. The catamenia began when she was 16 years old; they have always been very scanty, lasting from one to three days; she has suffered pain both during and between the periods; the pain is such that she has to lead more or less the life of an invalid, and it has increased in severity for the last two years. On examination, the uterus is small and normal in position; the right ovary can be easily palpated, and feels about half the normal size; the most careful examination failed to detect the left ovary. In this case I suggested that electricity might be tried for a couple of months with a view of endeavouring to stimulate the ovaries; but if (as was most probable) this failed, then, provided the patient suffer sufficient pain to justify her running the risk, the only course left was removal of the appendages.

When mentioning electricity, I meant its application by the introduction of one pole into the uterine cavity, and placing the other on the abdomen: *not* by using the so-called galvanic stem pessary; for all stem pessaries are fruitful sources of pelvic inflammation, and should only be used on the rarest possible occasions, whilst with regard to this particular variety I think most gynæcologists consider it absolutely worthless as a generator of electricity.

"Apoplexy of the ovary," or, as it might more appropriately be termed, "hæmorrhage into the ovary," is a condition which has been described by various observers, especially by Winckel and

Olshausen. Olshausen mentions two varieties: (*a*) hæmorrhage into the stroma, and (*b*) hæmorrhage into the follicles. That the condition cannot be so rare as is usually considered is seen from the fact that it occurred four times in my thirty cases. When an ovary is diseased (as in Case 27, in which it is tubercular) it can be readily understood that extravasation of blood into the stroma might without difficulty take place; that such an extravasation, even though of limited extent, can give rise to great pain, collapse, and fainting was well exemplified in the same case, for on Dr. Steele's arrival he found the patient so collapsed that he had to inject ether. In Case 26 the hæmorrhage apparently took place into an enlarged follicle and was associated with an effusion into the Fallopian tube of the same side. Probably some extraneous cause in this young married lady, such as coitus close upon the onset of the catamenia, induced excessive hyperæmia of the ovary and tube, resulting in hæmorrhage into these organs.

The *symptoms* of diseased appendages are essentially two—viz., pain and menorrhagia. The *pain* is variable: in some cases it may only come on at the periods and then be quite bearable; in others the dysmenorrhœa amounts to intense agony. As a rule, in these cases there is more or less constant pelvic pain, which increases in severity for about ten days or a fortnight before the period comes on, reaching its height at this time, and diminishing after the flow has been fully established. The pain is referred to the side most affected; it is often described by the patient as of a gnawing, tearing character, shooting through to the back, and perhaps down the thigh to the knee. In some instances it has been described to me by patients as feeling "as if someone had seized hold of their insides and was trying to tear them out." In my experience the greatest amount of pain is felt as a rule in those cases where, from extensive pelvic peritonitis, the ovaries and tubes are matted down in the pelvic cavity. In these cases a vaginal examination may reveal nothing beyond some impaired mobility of the uterus with tenderness on pressure. Usually in chronic disease of the appendages there is pain on coitus, and this may be of all degrees of severity; not a few of my patients have told me it was so intense that they have been compelled to lead a single life for months.

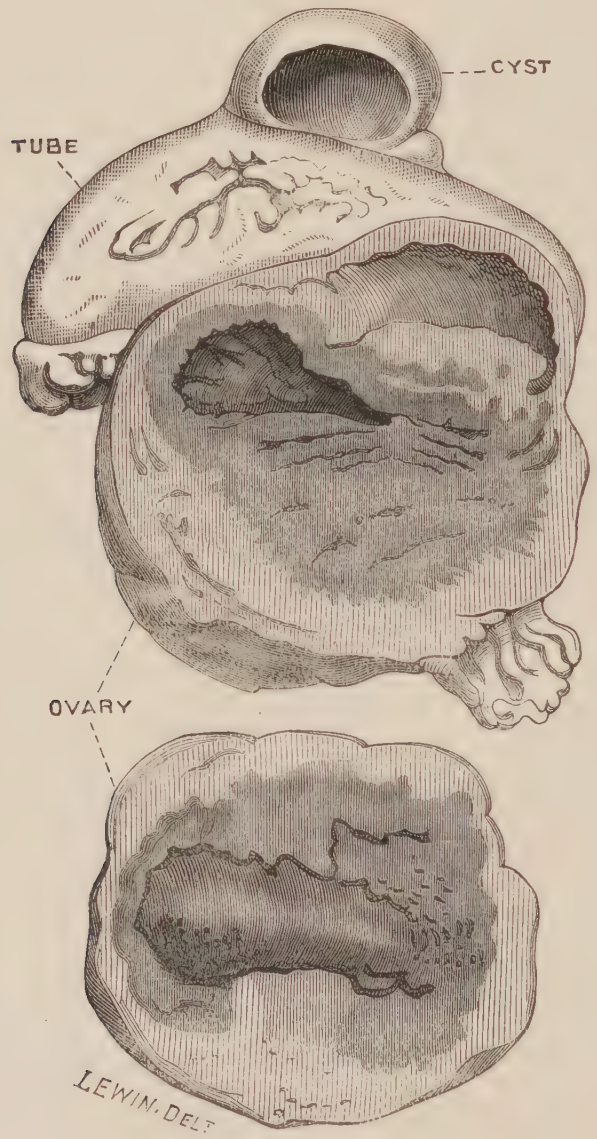
The *hæmorrhage* also varies in amount. In most cases the loss at the period is increased; women will say that their periods,

instead of lasting four or five days, now go on (perhaps) for a fortnight; that, whilst half a dozen diapers used to be sufficient, now they require from eighteen to twenty-four; others will say with regard to their periods that "they are hardly ever free from them." I believe the loss is greatest in cases of chronic ovaritis and salpingitis; but where the tubes are dilated with serum or pus there does not appear to be much alteration in the amount lost. Another symptom to be remembered is, as already mentioned, the occasional discharge "in gushes" of water or matter in those cases of intermitting hydro-salpinx or pyo-salpinx.

The *physical signs* are of the greatest importance. There is a good deal of difference of opinion respecting them; some able observers never, or hardly ever, find diseased tubes and ovaries on examining their patients, whilst others are constantly diagnosing them. In many instances the signs are sufficiently obvious on making an ordinary bimanual examination, whilst in others it is essential to give an anæsthetic and to examine *per rectum* as well as *per vaginam*. Can a normal ovary be palpated when in its usual situation? I answer, unhesitatingly, Yes, in the majority of cases where the abdominal walls are thin and lax; not only so, but atrophied or congenitally ill-developed ovaries can be made out; so also can the Fallopian tube, especially near the uterine end. When the tube is thickened from chronic salpingitis and is about as large as one's little finger, it feels not at all unlike a varicocele in the male, giving the same sensation to the finger that a bag with a few worms inside does. Not infrequently the ovaries and thickened tubes are prolapsed into the pouch of Douglas; then their detection is possible even to the *tactus non eruditus*. When enlarged ovaries and tubes are matted together by old pelvic adhesions (see Fig. 9), they give rise to a firm roundish or elongated tumour, felt in one or other posterior quarter of the pelvis, and perhaps extending somewhat behind the uterus. It is only within comparatively the last few years that the pathology of these tumours has been rightly understood. In my student days they were almost invariably described as cases of pelvic cellulitis in one or other broad ligament; but, since abdominal section has been more frequently performed, it is clearly proved that cellulitis does not exist except as part of a septicæmia or as secondary to a pelvic peritonitis. Those tumours, then, which are detected at one or other side of the uterus, and which do not depress the vaginal

roof or materially displace the uterus, are in the vast majority of cases the result of pelvic peritonitis following upon chronic disease of the ovaries and tubes, whereby these organs are matted together

FIG. 9.



Chronic peritonitis.—Ovary and tube matted together; ovary (shown in section) much enlarged and full of blood; tube dilated.

and to the different structures in their vicinity, such as broad ligament, uterus, omentum, and intestines. When a hydro-salpinx or pyo-salpinx exists, it feels (in a typical case) an elongated tortuous elastic swelling, extending from the side of the uterus outwards in the broad ligament, and backwards into Douglas's

fossa. In many cases, however, the dilated tube is roundish instead of being elongated. This is probably due to adhesions. On the other hand, there may be a large hydro- or pyo-salpinx which the most careful examination under an anæsthetic fails to detect. This is due, I believe, to the fact that the dilatation is not a tense one. In two cases on which I operated for disease on one side, I found in each, after removing the affected parts on that side, a large dilated tube on the other, which had previously escaped detection. When the ovaries and tubes are matted to the pelvis by adhesions, examination *per rectum* is very important. By this means also we can ascertain whether prolapsed ovaries are free or adherent.

The *prognosis* depends on the conditions present in any individual case. A woman with a pyo-salpinx is in constant danger of its setting up acute and fatal peritonitis; even a hydro-salpinx may do the same, although the risk is much less. In most of the other conditions, whilst they are not necessarily fatal, they as a rule render life one long state of chronic invalidism, accompanied by greater or less suffering, which may certainly be alleviated by medicines, but which, I fear, can usually only be cured by abdominal section. When this operation is resorted to, the prognosis is very good, as seen in the thirty cases recorded, where there was only one death, and this from the distressing accident of slipping of the pedicle ligature and fatal hæmorrhage. The ultimate prognosis as to sound health is also good, but it must not be forgotten that the full benefits of the operation are not usually felt for several months after, as the patients have to pass through the sympathetic disturbances incidental to the bringing on of the menopause.

Treatment.—1. When a definite tumour of the appendages is found in the pelvis of a woman who complains of pain and perhaps hæmorrhage, I am strongly of opinion that the condition should, as far as possible, be described to her, and abdominal section recommended after the risks attaching to it have been fully explained. 2. If the tumour be a dilated tube, the same course should be adopted, as even a hydro-salpinx may set up peritonitis. The old methods of treating dilated tubes, either by tapping *per vaginam* or by electricity, are not only unscientific, but also more dangerous than abdominal section. In any case where the symptoms and physical signs lead one to suspect tubal gestation, it is

our duty to insist on operation without delay. 3. In cases where the tubes are thickened from chronic salpingitis, with perhaps the ovaries enlarged and tender, or even prolapsed, tonics, aperients, repeated blistering, hot vaginal douches, and glycerine tampons should be tried for a couple of months, and if at the end of that time there be no improvement, then I do not hesitate to advise the removal of the appendages, provided the patient suffers sufficiently to run the risk of the operation. 4. If after one or more attacks of pelvic inflammation a woman suffers pelvic pain, dysmenorrhœa and dyspareunia, and perhaps menorrhagia, the great likelihood is that careful pelvic examination will detect the ovaries and tubes matted together and to the surrounding structures and the mobility of the uterus impaired, but where probably no definite tumour can be made out. What should be our line of practice in a case of this kind? Are we to pin our faith on Epsom salts, like some do, and when this drug fails to fold our hands, express the deepest sympathy with the sufferer, but on no account suggest any operative interference? Surely, this practice is just as reprehensible as the opposite one, where the appendages have been removed without obvious disease, but for some neuralgic affection. From personal experience of these cases, I know that nothing short of abdominal section will cure, that the operation is usually extremely difficult, and that the ultimate result is correspondingly satisfactory. 5. In cases of congenitally ill-developed ovaries with an acutely anteflexed uterus associated with marked dysmenorrhœa and sterility, I would first under anæsthesia widely dilate the cervical canal by means of Hegar's dilators; this failing, electricity might be tried, and as a last resource nothing is left to be done but removal of the diseased ovaries. With regard to the question of unsexing a woman by the removal of her appendages, it need not influence our treatment in the conditions mentioned under the above five headings, seeing that she is already unsexed, as the appendages are practically useless as far as procreation is concerned; and the subsequent history of those of my patients who are married bears out the statements of other observers that the sexual appetite is not usually diminished by the operation; indeed, in some cases the opposite result ensued. 6. Whenever on opening the abdomen the operator finds the ovaries and tubes fixed by adhesions, he should break down the latter with his fingers, and then, having brought the appendages into view, carefully examine

them for any well-marked visible or tangible signs of disease. If no marked departure from the normal be made out, then it is the operator's bounden duty to stay his hand from removing the appendages. I firmly believe that the cause of the patient's symptoms is, in not a few cases, the presence of adhesions, and that when these are broken down it is unnecessary to proceed further; but how many operators have the moral courage to do this? One or two dilated follicles seen on the surface of an ovary are thought sufficient to justify the spaying of the patient. 7. Inasmuch as there is usually an absence of disease of the appendages in cases of ovarian neuralgia occurring in neurotic subjects, any local interference is not only unnecessary, but absolutely unjustifiable, and the same remark applies in great measure to cases of hystero-epilepsy and other mental disorders.

It cannot, I think, be too strongly enforced that removal of the appendages for chronic disease is in most cases much more difficult than performing ovariotomy; indeed, in a very small proportion the operation may have to be abandoned, as occurred to me in Case 21. Hence the tyro in abdominal surgery ought not to attempt operating on these cases. Again, it is most essential that the patient and her immediate relatives have the nature of the operation and its risks fully explained to them, as well as the change in life which will be produced by it.

Before concluding, I wish to make a few remarks on the "technique" of the operation.

Anæsthetic.—With regard to this, almost every operator has a predilection for a particular one. Until comparatively recently I preferred my patients to have ether, but now it seems to me that chloroform given either on a piece of lint or by Junker's inhaler is the best, as it produces more complete relaxation of the abdominal parietes, and there is no risk of lung complication following. A few years back a patient on whom I performed ovariotomy died two days later from acute bronchitis induced by the ether administered.

Antiseptics in the Listerian sense of the term are unnecessary in abdominal operations; indeed, I would go further, and say that the spray may be positively injurious. The great thing is strict cleanliness. The routine practice I carry out is as follows:—Having thoroughly washed my hands and forearms in soap and

water, using a nail-brush vigorously, I then dip them into a 1 in 2,000 solution of corrosive sublimate for one minute. The patient's abdomen is scrubbed with soap and water, and afterwards washed with the corrosive solution. The sponges are wrung out of boiled water, and boiled water is poured over the instruments. Nothing else, except that my assistant treats his hands and arms in the same manner as I do. One great element of success in these operations is that, if possible, no fingers but the operator's should be allowed to enter the peritoneal cavity, and on no account whatever should any bystander examine the case or give help until he has rigidly rendered his hands aseptic.

Ligaturing the broad ligament in these cases requires more care than the pedicle in ovariotomy, and I believe it would be safer in all cases to tie it in three portions with strong silk (Chinese twist). The Staffordshire knot seems not to be so safe as the ordinary method of dividing the double ligature and tying in two parts after crossing the threads. A few weeks back I very nearly lost a patient from hæmorrhage after ovariotomy. The pedicle was rather thick and long, and I thought I would show Dr. Noakes, of Nice, who was present, the ease with which the Staffordshire knot can be applied. Having removed the tumour, cleansed the peritoneal cavity, and tied all the abdominal sutures but two, I saw a little blood at the bottom of the incision. On separating the edges, sure enough the abdominal cavity was full of blood. I rapidly seized the pedicle, and found the ligature had slipped; so, after retying securely, I washed out the peritoneal cavity as quickly as possible with a 5 per cent. boracic acid solution, removing in doing so large clots from near the liver and spleen. Had half a minute more elapsed I should have closed the abdomen, and most likely the hæmorrhage would not have been discovered, as the patient was in a very feeble and collapsed condition all through the operation; as it was, she rapidly recovered without a bad symptom.

Drainage.—A good deal of difference of opinion exists on the value or otherwise of drainage. Personally I never use a drainage tube in simple cases, but where numerous adhesions have been torn through, causing free oozing of blood, or where some of the contents of a tumour have escaped into the abdomen, then I insert a glass tube for from thirty-six to forty-eight hours, and with the happiest results. One disadvantage of using the drainage tube is that that part of the abdominal wall through which it passes

seems to unite less firmly than the rest, so that later on the scar has a tendency to give, causing a ventral hernia.

Flushing the peritoneal cavity is, I think, a proceeding of risk; at the same time it is very valuable in cases where pus or other irritating fluid escapes into the cavity. I always use a 5 per cent. boracic solution at a temperature of 100° F. introduced through a large indiarubber tube attached to a douche tin. It is necessary after flushing to use a drainage tube. The immediate effect on patients varies: some appear stimulated and others markedly depressed, but I know no way of finding out beforehand which will happen in any given case.

The *after-dressing* is important. The abdomen having been thoroughly cleansed with a sublimate solution, I place over the wound a strip of oil silk which has been dipped in the solution; this strip is about an inch and a half wide, and long enough to extend a little way beyond the extremities of the incision. Boracic acid is next dusted over the edges of the silk and over the rest of the abdomen; plenty of sal alembroth or salicylic wool is next laid on, and the whole firmly fixed by wide strips of adhesive plaster, reaching from below the hips up to the epigastrium. It will be noted that nothing but the oil silk touches the wound; this, I find, gives every satisfaction, and when the first dressing is removed on the eighth day it causes no pain to the patient. Formerly I used to dust iodoform powder over the wound, but it is not free from risk. I had a succession of cases in which troublesome suppuration took place in the abdominal parietes along the tracks of the stitches; this I could not account for nor prevent until I discarded the iodoform, when the succeeding cases progressed in the normal manner. In other cases it produced delirium.

After-treatment.—My plan is the following:—Nothing whatever passes the lips for twenty-four hours; but if the thirst is distressing I have the mouth washed with warm water frequently, and, provided there be no sickness, a teaspoonful of hot water given every two hours. I believe ice is not so good as hot water, and it certainly produces flatulence. In cases where the operation has been very severe, or in elderly women, a nutrient suppository is administered *per rectum* every four hours. If there be decided pain, a one-third grain morphia suppository is given and repeated when necessary. Morphia should not be given as a routine practice, and many cases do perfectly well without it; but I think it is

distinctly cruel to deny it, as some do, to patients suffering decided pain. After twenty-four hours an ounce of milk and soda is given every two hours, increasing gradually, and with some beef-tea the next day. As a rule, a small cup of ordinary tea with milk is longed for by, and most grateful to, the patient. The urine is drawn off every eight hours for a few days, but when patients have a desire to pass it naturally (even two days after the operation) I always allow them to do so. An aperient is given on the seventh day (before the stitches are removed), and the patient gradually takes solid food. She is turned on the side occasionally after the seventh day, and at the end of ten days she is allowed to sit up in bed. So much for ordinary cases. When complications arise, the treatment requires modification. *Vomiting* is the most common, and the ordinary sickness from the anæsthetic is best treated by letting nothing be taken for twenty-four hours. Recently I have adopted (and with distinct success) a plan of Mr. Howse's suggested to me by Dr. Moss, the able resident medical officer at the Royal Hospital for Women and Children. It is to administer *per rectum* at the termination of the operation an ounce of port wine in an equal quantity of hot water. In some cases there may be severe vomiting, not from any inflammatory condition of the peritoneum, but probably from mere atony of the bowel. If in addition to the vomiting there be flatulent distension of the abdomen, the case simulates closely one of peritonitis, and it is here that an aperient of sulphate of magnesia or soda acts like a charm. One of my patients (Case 9), a thin, neurotic lady, had frequent vomiting for six days after the operation (on four days it was black) which nothing would allay. At last I thought I would give an aperient, but as she was very low I called in consultation an eminent obstetric physician, who did not think it advisable to purge. Next day, however, as she continued getting worse, I gave her a dose of white mixture, and repeated it every two hours until the bowels acted. The effect was marvellous. She was only slightly sick, once after the first dose, and from that time recovered steadily. *Flatulence* is a frequent and often troublesome complication I make it a routine practice to have a soft indiarubber tube passed into the rectum for several inches at least twice a day, and find it gives relief. In a case of Porro's operation which I performed at the Middlesex Hospital (and reported in the 'Lancet,' vol. i, 1889, p. 16) there was enormous flatulent

distension a few days after the operation, giving rise to all the signs of acute peritonitis, but on passing a long stiff tube into the colon a tremendous escape of flatus put an end to the unfavourable symptoms. In slight cases a teaspoonful of brandy in half an ounce of hot water gives much relief. My patients suffer little from flatulence since I have discarded the use of ice. *Peritonitis* is the only other complication about which I shall speak. It may, of course, have existed before the operation, or it may be due to the escape, at the time of operation, of some pus or irritating fluid into the abdominal cavity; but when it sets in after a straightforward case of abdominal section I cannot help thinking that the operator has to reproach himself that the measures he adopted to ensure strict cleanliness were wanting in completeness. Although when once it has set in it need not necessarily be fatal, I believe the best chance is given to the patient by reopening the wound without delay and irrigating the cavity thoroughly with the boracic acid solution and inserting a drainage tube. The only case in which I did this for acute peritonitis coming on two days after I had performed abdominal section recovered completely, and is now, three years after, perfectly well. Mr. Lawson Tait recommended the use of saline aperients, but I cannot help thinking that even such an experienced and able observer as he may have mistaken distension and vomiting for peritonitis.

In conclusion, I would remark that simple cases of abdominal section seem to recover equally well if put back immediately after operation into the general ward as when isolated in a special ward for several days.

THE TREATMENT OF CHRONIC DISEASE OF THE UTERINE APPENDAGES.

By Alban Doran, F.R.C.S.

My object in reading this essay is to review briefly the subject expressed in its title. I purposely avoid the introduction of original matter. At the same time, I refrain from all purely analytical subjects. Clinical records would be out of place in this paper. Statistics are equally unsuited for my purpose. In discussing one particular method of treatment, statistics may form

the title-deed of the speaker's experience. In reviewing all methods, the sources of statistics are of relatively little value. Thus, by the aid of series we cannot fairly compare such an operation as salpingostomy, which has been performed two or three times by two experts, with oöphorectomy, which has been largely practised by innumerable specialists, surgeons, and practitioners.

Treatment by rest, which is, relatively speaking, expectant, cannot be satisfactorily judged by statistics. It involves essential factors very hard to tabulate. Such a task demands special mental gifts and opportunities such as enabled Dr. Boxall to prepare, at the cost of enormous labour, his invaluable tables relating to pregnancy and childbed. On the other hand, the tabulation of operations, with the results and after-histories, is work which most men may, with due deliberation, undertake. We do not yet know how to reduce our "rest" cases and operation cases to equal terms, in a mathematical sense. We must content ourselves therefore with a brief review of all forms of treatment.

Throughout this memoir I will, except when I specify to the contrary, speak of chronic tubal and ovarian disease as one complaint, "disease of the uterine appendages." Their distinct pathology cannot be discussed.

The specimens which I now exhibit will demonstrate the usual varieties of chronic disease of the appendages.

I cannot dwell upon the sources of chronic disease of the appendages. The acute forms do not come within the scope of this memoir. Before discussing the treatment of chronic disease in detail, I will, for purposes of convenience, classify the different remedial measures now more or less in vogue. They are :—

1. Rest.
2. Electricity.
3. Massage.
4. Curetting of the uterus, and Emmet's operation.
5. Catheterism of the tubes.
6. Vaginal puncture of cystic tubes.
7. Abdominal section, which includes the following subdivisions :—

a. The complete operation, removal of the tube and ovary, or oöphorectomy.

b. Drainage of a hydro-salpinx.

c. Breaking down of adhesions and drainage.

d. Essentially "incomplete operations," or simple opening of the peritoneum.

e. Plastic operations on the tube, or salpingostomy.

Rest is in itself sufficient to cure most recent cases, especially if they have not been aggravated by injudicious therapeutic measures. The exceptions are chiefly those rare instances where suppuration of the tube or abscess of the ovary sets in early. Therapeutic measures of a kind familiar to every practitioner must not be neglected, but must not be frivolously undertaken. The effect of drugs and applications should be carefully watched. Saline aperients are usually beneficial, yet in some of the mildest cases they may cause prolonged abdominal pain with rise of temperature, probably through disturbance of intestinal adhesions. I saw this occur last spring in a young woman, aged 24, with symptoms of disease of the appendages. Tympanitic distension of the abdomen remained for several weeks after the subsidence of the pain and fever. Now, early in November, I removed two ovarian cysts with twisted pedicles from a middle-aged patient. I had to separate abundant intestinal adhesions, and small patches of the serous coat of the ileum were unavoidably stripped off. On the eighth day I gave a drachm of sulphate of magnesia; and severe pain, rise of temperature, and tympanites rapidly followed, and did not subside for several days. I have no doubt that the former case suffered from the same complication, disturbance of intestinal adhesions around pelvic structures. Both patients were afterwards treated with enemata, and henceforth gave me no trouble.

Again, the tampon sometimes sets up severe irritation. The uterine cavity must, as a rule, be left alone. Frequent passing of sounds, and dilating of the os, will certainly aggravate the disease, and may set up fatal complications.

Care must be taken when rest is prescribed that the patient really rests. To tell a patient to lie up for an hour or two in the middle of the day, and then to cut off her ovaries a month or two later, because she reports herself as no better, is wrong. The patient must lie in bed, and, while perfect repose is insured, the attendant must also steer clear of the danger of making her a chronic invalid. The double task is difficult. Friends must not be allowed to bring her unprescribed delicacies; nor, on the other hand, must she be kept on slops.

How electricity can cure diseased appendages is a mystery which

it is for those who practise the method to explain. In alleged cases of recovery, it is the subjective symptoms which have been relieved. Dr. Kehrer has shown ('Centralbl. f. Gynäk.,' 1889, p. 736) that electricity applied to the cavity of the uterus may cause the rupture of a suppurating Fallopian tube. The application of electrical appliances involves a considerable amount of handling. Nothing bears handling so badly as a suppurating tube or ovary. This handling, whether for diagnosis or treatment, may not kill, but it is almost certain to aggravate the disease. Anæsthetics may relieve the pain, whether from electricity or massage, but they encourage very rough handling of the diseased parts. Electricity *may* check the growth of fibroids, but that subject is not to the point. It *may* cure gonorrhœa by killing the gonococci; indeed, Dr. Prochownick declares that he has slaughtered those germs by the galvanic current.* This statement sounds wild, yet it at least claims a definite action. In disease of the appendages, it is hard to understand how the current can possibly open tubes, clear adhesions, and remove the products of inflammation.

There are obvious objections to massage when practised upon the genitals. The authorities who advocate it must be much at variance with those operators who remove cystic tubes to anticipate rupture and peritonitis. It is very reasonable to fear that the manipulations of massage may burst a distended tube. Dr. Koplik, of New York,† believes that massage is indicated in many forms of disease of the appendages. He considers the employment of this method unjustifiable in all cases where the tubes feel in the slightest degree dilated or the ovaries uneven, that is, in almost the entire class now under consideration. He admits that diagnosis is very difficult. In a woman, aged 31, where he diagnosed chronic parametritis, a profuse purulent discharge from the vulva followed massage. Examination showed a marked diminution in the thickness of the adhesions posterior to the uterus. Dr. Koplik surmised that a collection of pus had found its way into the body of the uterus. The emptying of a pyo-salpinx seems a more probable

* 'Münchener medicin. Wochenschrift,' No. 27, 1890. The uterus bears a current up to 120 milliampères, which is strong enough to kill the gonococci; but the urethra, according to Dr. Prochownick, can hardly tolerate 40 milliampères, so electricity is useless for the purpose when the urethra is affected.

† "A Contribution to the Literature of Massage of the Uterus and Adnexa," 'Amer. Journ. Obstet.,' vol. xxii, 1889, p. 136.

explanation. Dr. Koplik thought that "the patient was fortunate to have escaped an exit of pus into the cavity of the peritoneum." We must all agree with him in his opinion, and we must feel that other patients may be less fortunate. Let it be granted that in any particular case pyo-salpinx may be accurately diagnosed by a clinical history and by palpation, who would be so bold as to deliberately attempt to squeeze the pus out of the tube into the uterus?

Certain gynæcologists, especially in France and Germany, trace inflammatory disease of the appendages to endometritis, and accordingly, after their custom, dilate the uterus and scrape the endometrium. The use of the curette in chronic salpingitis may mean the sacrifice of the patient to a theory. Its advocates maintain that there is a disease called endometritis, and that in certain forms of the complaint the endometrium is converted into a fungous surface which discharges muco-pus and sanies. Being reproduced, like the normal endometrium, at each menstrual period, the traces of the diseased structure infect its successor. Conception is impossible and, what is more to the point, the morbid discharge gets into the tubes and sets up salpingitis. Let the diseased endometrium be scraped away, and let the uterine cavity be well swabbed with antiseptic lotions, and the case is cured. A healthy endometrium can now form, the uterus will hold a fertilised ovum, and, should tubo-ovarian disease exist, the cause being removed, it will soon disappear.

Such is the teaching of the advocates of the curette. Unfortunately it is based on assumptions and on confusion of cause and effect, and is practised recklessly by many who are taken in by pretty French words like "curettage," "hersage," and "écouvillonage." The precise physiology of the endometrium is far from being clearly understood. Part of its substance is certainly shed at each period. The doctrine of the production of a new diseased endometrium as a necessary consequence of disease of the old endometrium previous to a given menstrual period is indeed hardy. Considering that other mucous membranes are not periodically shed, it seems strange that the uterus should be worse off than the respiratory or alimentary tracts, when its mucosa is diseased. Applying this doctrine to those tracts, it is to be wondered how bronchitis or enteritis can ever undergo spontaneous cure.

Yet the endometrium certainly *is* unhealthy in some cases, and

the scraping *may* in such instances prove beneficial. We cannot, however, assume that true endometritis sets up tubal disease. Should disease of the tubes be present, few proceedings can be more dangerous than dilatation and scraping. Tissues are thereby irritated in the neighbourhood of inflamed pelvic structures, and all for an unsettled theory. On the other hand, there can be no doubt that the curette has been the direct cause of severe inflammation of the appendages. Whatever may be the value of that instrument, it must be discarded in the class of cases now under consideration.

There are some gynæcologists who fancy that laceration of the cervix is the source of wide-spread evils, inflammation of the appendages included. On this theory, they advocate Emmet's operation, or trachelorrhaphy. I do not myself believe that a laceration of the cervix is in all cases absolutely innocuous, but there is little evidence that it can play a conspicuous part in causing inflammation of the tubes and ovaries. Cases of that disease which have recovered after repair of a laceration most probably owe their cure to the enforced rest which follows that operation.

Catheterism of the tubes in order to employ them is a fine idea, in theory. In practice it cannot well be carried out. The uterine orifice of the tube is not to be reached with surety or safety. Should a sound be passed into that orifice by chance or skill, the dangers of pushing it, in the dark, along the canal of the tube remain evident. No attempts to open the ostium would be justifiable, whilst if accidentally opened the results might be grave, for the sound might introduce morbid materials into the peritoneal cavity. Neither the ostium nor the uterine end of the tube would necessarily remain patent after successful catheterism. Lastly, the tube might be healthy, or at least patent, whilst the disease lay chiefly in the ovary ; then catheterism would be useless.

Vaginal puncture of the cystic tube has been strongly recommended by Leopold. He maintains that, even if the tube be not completely emptied by the puncture, its muscular coat can once more act, just as the uterus contracts if the membranes be ruptured and the waters escape during pregnancy. Above all, this proceeding avoids mutilation. Leopold, however, honourably admits that puncture sometimes fails to cure, and confesses that he has wounded cystic tumours and fœtal sacs by mistake. To push a

supposed cystic tube downwards by pressure on the abdominal walls, and to puncture through the vagina, is indeed operating in the dark. As in the case of catheterism, the ovaries may be the main seat of disease.

If the reader wishes to study an example of all that should be avoided in the treatment of an advanced case of disease of the appendages, he should carefully peruse Professor Reverdin's honest report of a case of "Pyosalpingitis with Perforation of the Bladder," in the 'Revue Médicale de la Suisse Romande,' November, 1890. Trocars and knives plunged into the swelling above and below nearly killed the patient by sepsis and hæmorrhage. Abdominal section, boldly carried out, just saved her life, which would never have been endangered had it been undertaken in the first instance.

Granting that rest and therapeutics fail, abdominal section is more satisfactory, and even safer, than the measures above noted.

Oöphorectomy is the best operation in a large class of chronic cases where subacute seizures occur frequently and at gradually shortening intervals, and where careful bimanual palpation proves the existence of a mass, usually tender, on one or both sides of the uterus. The tube and ovary are degenerate and useless. The more cystic they become the more discomfort they cause, and the more probably will they form adhesions to intestine, omentum, &c. Pyo-salpinx, a not unfrequent complication, is in itself a source of danger to the patient.* The health suffers, the patient is crippled, and, if poor, incapacitated from earning her bread.

There are several grave considerations in respect to oöphorectomy. The operation is most difficult in those cases where it is most justifiable. The surgeon who has triumphantly performed half a dozen ovariotomies is dismayed when he finds, on attempting oöphorectomy, how unfamiliar to his touch are the affected parts, hidden deeply in the pelvis, soldered together, and often intimately adherent to the intestines. The abdominal wound involves trouble, the omentum frequently adheres to the parietes, and the intestines are hard to get out of the way. Nor does the wound always heal kindly. Hernia of the cicatrix is more frequent than after ovario-

* Especially when there are fibroid growths in the uterus. See Adenot, "Salpingite Suppurée Double; Péritonite Suppurée; Mort Subite," 'Archives de Tocologie,' November, 1890. In many suddenly fatal cases of suppurative peritonitis, pyo-salpinx may be overlooked.

tomy. This appears to be in part due to the fact that, the abdominal walls not having been distended by a tumour, the muscles are of normal power, and frequently contract with force during convalescence. This seriously interferes with healing. The intestines, when distended with flatus, can act more directly on the wound than after ovariotomy, when the flaccid walls allow of more room for distension, and can readily be supported by bandages and pads. This pressure from within, and not muscular action, is the great enemy to sound cicatrisation of the wound. Again, there is another objection to oöphorectomy. The painful symptoms are not always relieved by that operation. This may be due to irritation of the ligatured stump; more often it depends on the presence of adhesions between adjacent coils of intestine. Dr. Coe, of New York, was among the first to turn attention to this fact. These adhesions may easily be overlooked, and, when detected, the operator is loth to disturb them. Oöphorectomy for the removal of diseased appendages is, in fact, difficult, and not always successful in the experience of the best operators. When incomplete, or clumsily performed, it is very dangerous.

Certain details of the operation must now be considered. The abdominal incision should not be made long; about $2\frac{1}{2}$ inches will, as a rule, be sufficient. A long incision is absolutely necessary for the removal of a large solid fibroid, and, when the tumour is extracted, there is little difficulty in preventing the prolapse of intestine. For there is plenty of room behind the now flaccid walls, and a large flat sponge may be slipped in with perfect ease. In removal of the appendages, a long incision cannot aid the operator in finding and extracting the ovaries and tubes; but it is certain to give him great trouble through prolapse of the intestines, which are normally, that is to say closely, packed in the abdominal cavity. The introduction of a sponge, necessary in every case, is somewhat difficult, and requires careful manipulation. The danger of hernia has already been mentioned. The incision must be brought down close to the pubes without damaging the bladder, for if the lower angle of the wound lies too high, manipulations deep in the pelvis will be awkward and difficult.

The after-treatment is the same as in cases where ovariotomy has been performed. In the Samaritan Hospital I have watched the recovery of a large number of cases in the practice of my colleagues and myself. As far as convalescence from the opera-

tion is concerned, matters usually proceed in a satisfactory manner. We rarely see the severe constitutional disturbance which sometimes follows oöphorectomy for bleeding fibroids. The two especial complications are suppuration of the pedicle and delayed healing of the abdominal wound. The pedicle of an ovarian cyst is generally made up of healthy tissues and experience shows that it tolerates the ligature, properly applied. The pedicle of an inflamed tube and ovary is not always healthy; it includes broad ligament infiltrated with inflammatory products, and the exposed unhealthy mucous membrane of the stump of the tube. Here lies a source of danger, and free suppuration occasionally occurs. The abdominal wound, exposed to the action of powerful muscles, not weakened as in ovariotomy by previous stretching, is often slow to heal. Nevertheless, as a rule, oöphorectomy for chronic disease of the appendages is followed by speedy convalescence.

Unfortunately, a permanent cure is not so frequent. The cases where the stump suppurates are particularly unsatisfactory. Fistulous tracts open, close, and re-open in the abdominal wound for months, discharging thin pus. Such cases find their way to the consulting-rooms of others, or to other hospitals than the institution where the operation was performed. The operator hears no more of them, and he, or the surgical registrar, records them in perfect good faith as "cures." A larger minority suffer from continuance of the pains which preceded the operation. The ill-success is not hard to explain. Sometimes inflammatory products, pressing on nerves, are, unavoidably, left behind. Sometimes adhesions between coils of intestine—the cause of the pelvic pains in these particular cases—remain undisturbed. The ligatures may set up fresh trouble, most commonly parametritis, fixing the uterus for awhile. Lastly, grave mental symptoms may follow oöphorectomy, especially when a neurosis exists. No doubt this neurosis is aggravated by the menstrual periods which the operation suppresses. But the artificial and premature menopause thus introduced may prove a far more severe shock.

Still the majority of cases of oöphorectomy do well. The patient is restored to health and comfort.

The simple draining of a hydro-salpinx is an operation that often proves highly satisfactory. Many large pelvic cysts, with a history of old inflammation, represent a dilated tube alone. But, whilst a true cystoma of the broad ligament is often shelled out of its

healthy capsule with ease, the enucleation of an old diseased and cystic tube may prove very difficult. Incision and drainage, on the other hand, are simple proceedings, and give the best results. The pain is caused by the tension of the vascular capsule, always increased at the menstrual period. Incision and drainage remove the cause of pain. A considerable part of the cyst and capsule should be trimmed away so as to prevent any chance of closure and refilling. One great advantage of incision and drainage and complete removal of the appendages is that no diseased ligatured stump is left behind in the pelvis, as is the case when a tube and ovary in an earlier and less degenerative stage of inflammatory disease has perforce to be amputated. The relief, after the opening of a painful tubal cyst, is intense. Most satisfactory cases of drainage of hydro-salpinx have occurred in my own practice.

Another less radical measure is abdominal section, with free breaking down of pelvic adhesions and drainage, the appendages (be it understood) being found relatively healthy, and the ostia of the tubes unobstructed. If the local adhesions be carefully broken down, the cause of suffering is removed. By appropriate treatment, for a few weeks after the operation, a recurrence of the inflammation may be avoided. Two considerations must be taken into account in respect to this proceeding, namely, the experience of others and the difficulty of the operation itself. We hear much about consecutive cases of removal of the appendages, but little about this simple breaking down of adhesions. Men do not like to record what may be set down as a "partial operation." They have an idea that it may be set down by their rivals as a failure. The most honest operator in these cases usually sets to work with a fixed notion that the tube and ovary is at the time the sole or chief cause of trouble. The cutting away of a tube and ovary is definite—even if difficult. Then, in the amputated structure there is something to show for one's trouble, and for the risks to which the patient has been exposed. In so many operations it is right to take something away, that surgeons very naturally tend to believe that it is wrong to take nothing away. Lastly, the patient's prejudices may give us great trouble, since, though she usually dislikes the prospect of any kind of amputation or mutilation, she, on the other hand, having undergone an operation, is often dissatisfied if she learns that nothing has been removed. Hence the surgeon should clearly explain beforehand, in all cases where he intends to operate, that

the abdominal section is intended to set right what is wrong and is not necessarily undertaken in order to remove anything.

The operation itself, however, the breaking down of adhesions alone, is not, as a rule, easy. It is indefinite. No set of rules can be laid down to guide future operators. It is only when operating that the surgeon can determine if the tube and ovary be not sufficiently diseased to require removal, whilst certain adhesions should be deliberately broken down. Most assuredly, in any case, there are adhesions that had much better be left alone; whilst to disturb others would be criminal. Experience can alone guide in these cases, and the surgeon who has not done many abdominal sections must not look upon this proceeding as a mild affair. He must not hold it as an intended preliminary to removal of the ovary, which, if the ovary be found, in his opinion, to be irremovable, will signify little or nothing—a little benefit and no risk. On the contrary, such an operator should remember that carelessly breaking down adhesions is far more dangerous than cutting away an ovary, and, as in the kind of case we are considering, he is doubtful about the propriety of oöphorectomy, it stands to reason that he had best leave everything alone, and close the abdominal wound. In breaking down adhesions, the operator must take the fundus uteri as a landmark. He is certain to take care lest he should mistake intestine for tube, but he will confuse himself if he take an inflamed and distended tube for intestine, especially if it should rupture and discharge fœtid, dark, pus resembling fæces. The operator can only ascertain at the time of operating the very conditions which demand this procedure, for, should the appendages be extensively diseased, of course they must be removed. The breaking down of adhesions, as a distinct operation, always necessitates, in my opinion, flushing of the peritoneum and drainage, for capillary oozing is free in these cases, and suppurating foci are sometimes opened up between coils of intestine. In my experience, it is precisely the well-flushed cases that have done best. The advantages of this operation over oöphorectomy are evident; as in drainage of a hydro-salpinx no ligatured stump is left behind. As the ovaries are not removed, there is no danger of the mental complications which follow their amputation.

I must repeat an observation which I have already made. Should the appendages be found to be extensively diseased, of course they must be removed. A dilated and obstructed tube, or

an ovary, enlarged and cystic, will probably be a greater source of danger after they have been disturbed from their adhesions than before. Both may contain pus, and a tentative incision involves as much risk as extirpation, and complicates the more radical proceeding should it be undertaken.

We must not omit some consideration of those instructive cases where the operation is absolutely incomplete, yet the patient is cured of her pain. From this class all operations where adhesions have been undesignedly broken down must be excluded, as they come under the kind of procedure just described. I now speak of cases where an abdominal incision is made, and no morbid condition discovered, no objective signs of disease of the appendages, and no adhesions. In other cases the operator finds numerous intestinal adhesions which he fears to disturb. In either of these circumstances nothing further is done beyond speedy closure of the wound.

These incomplete proceedings are not popular amongst operators who tend to believe that they are stultified should a patient get well after "nothing" has been done. Hence these cases are seldom published. Yet I have known of more satisfactory results after these purely incomplete operations than after removal of the appendages. The latter may make a patient absolutely worse. The former may prove a perfect cure.

Is it "nothing," this simple opening of the abdominal cavity? Does the handling of the appendages perchance set up some salutary change, whether through mild stimulation of limited products of chronic inflammation or purely through nerve influence? Is the cure mainly due to the mental impression of the operation? We will put aside the possible effects of rest necessitated by the operation, for these cases have often been unsuccessfully treated by enforced rest already. Yet no doubt the repose is a factor in the cure. It is certain that where chronic peritonitis is present simple incision of the walls has the most frequently proved beneficial, especially when the parietal peritoneum is much thickened, as in a case under my care four years ago. The cutting through the thickened and vascular serous membrane appears to relieve the whole peritoneum.

There remains a somewhat difficult operation of an entirely conservative character, first advocated by Dr. Skutsch, of Jena, and Dr. Martin, of Berlin, within the past two years. Both claim good

results. This operation is salpingostomy, or the fashioning of an artificial ostium. A kind of button-hole is cut in the end of the tube, and the mucous and peritoneal surfaces are sewn together. This operation is not justifiable in pyo-salpinx, and not likely to succeed if any active inflammatory process be present, since the new ostium must, in such a case, be soon covered over by fresh bands. Salpingostomy seems to be a step in the right direction, especially in hydro-salpinx.

In conclusion, Mr. President, it is evident that removal of the appendages is an operation to be avoided whenever possible. In long-standing, neglected cases it is hardly avoidable, but, even then, one of the less severe operative measures, already indicated, may suffice. Already neglect, as the result of professional ignorance, is becoming rare. In this country, practitioners and nurses are growing more and more skilful in the management of lying-in women. Hence serious pelvic inflammation, even after abortion, is becoming uncommon. When it occurs it is generally detected and treated in its earliest stages, and then complete recovery is almost certain. Unfortunately, a large number of women will always neglect themselves, or else be exposed to neglect. Some of the more serious forms of inflammation, especially the gonorrhœal type, involve so much local damage that it is hard to see how an era can ever arrive when there will be no more patients who would be the better for the removal of their diseased and useless tubes and ovaries. I hope, on a future occasion, to be able to analyse a series of cases within my own practice, especially in respect to the relative results of complete and incomplete operations.

Professor SINCLAIR, of Manchester, pointed out that severe attacks of puerperal perimetritis were a frequent cause of the affections for which these operations had subsequently to be undertaken, and, as these had decreased of late years in consequence of greater care and skill in operations and instrumental labours, the number of cases of disease requiring operative interference had tended to diminish. He observed that little had been said of gonorrhœa as a cause of severe forms of the disease. In relation to this subject he related an anecdote of a student of his, a man who knew his work well, who, on presenting himself for examination in London, had been asked the most frequent causes of ovaritis, in answer to which he had mentioned gonorrhœa. Thereupon the examiner retorted: "I am talking of respectable married women, and not of strumpets." He urged that strumpets were not the only people who suffered from severe attacks of gonorrhœa; on the contrary, they took care of themselves, whereas the respectable married women, not being warned, frequently fell victims to infection from their husbands. Another frequent cause of

these diseased conditions was ill-advised treatment, especially at the hands of the younger men who had obtained a smattering of gynæcology, and were but too anxious to display their skill. He instanced the case of a weakly married woman who had applied to a newly-fledged practitioner on account of a sore throat. The practitioner elicited the fact that, though married, she had never been pregnant, and thereupon he obtained permission to examine her, declared that the uterus was retroverted, and forthwith inserted a pessary. In the sequel the woman developed a hæmatocele, followed by abscess in the left broad ligament and Fallopian tube which burst into the rectum. He hoped someone would be bold enough to condemn this tendency to rash treatment on the part of general practitioners. He alluded *en passant* to cases of intermitting hæmatocele, and mentioned an example of this affection. The patient had been under his observation for several years, but as the tumour invariably cleared up after treatment, he had been unable to ascertain the condition of the parts. He had frequently operated for removal of the tubes during the last ten years, but he had found it a difficult and dangerous operation. With regard to prognosis, he said there were operators who assured their patients that the operation was practically unattended with danger, and he mentioned a striking example of the folly of so doing. He urged that the Staffordshire knot ought to be abandoned as utterly unreliable, as much so, indeed, as Keith's clamp for ovariotomy. Lastly, he expressed unreserved approval of the practice of flushing the peritoneum, and expressed his surprise that Sir Spencer Wells should have obtained his results in view of the fact that he mentioned in a recent speech that he had resorted to flushing on only two occasions. In conclusion, he said that operators were disposed to regard the woman's escaping with her life as constituting *per se* a satisfactory result; but he urged that more attention should be paid to the ultimate effects upon the general health.

The President (Mr. Knowsley Thornton) said that, before proceeding to discuss in detail the various propositions which had been brought before them in the papers which had been read as introductory to the discussion, he must recur for a moment to the question which he asked at the last meeting, and to which he thought Dr. Duncan gave a very imperfect answer. He had elicited from Dr. Duncan that the table of cases now before them did not represent his whole practice in this operation, and that his results at some antecedent period were so unsatisfactory that he was not prepared to publish them; but he (the President) did not elicit the reasons which made Dr. Duncan fix upon these particular thirty cases to tabulate for their instruction. Now, whilst not a great believer in the value of these statistical records, because it is impossible in them to show the factors which in the individual cases may lead to success or failure, still he thought it was of some value to have the whole experience of any individual operator, and to watch and learn from it how far he had learned his work before he ventured to risk the lives of patients entrusted to his care, and how great was the amount of failure necessary before he attained fair success in operating. Such a record, he held, had a certain value, and so had the records of large masses of cases operated upon by one operator, which can be compared with similar masses operated upon by another of like experience. He thought, however, a limited list, such as that laid before them by Dr. Duncan, without anything to guide them as to dates, and without anything to guide them as to the reasons of limitation, was not only valueless, but was likely to mislead, and not only to give an entirely false impression as to his own practice, but to lead to

far more serious mischief, inasmuch as it gave a false impression of the dangers of these operations, and encouraged inexperienced men to attempt operations they were totally unfit to perform, and thus added to the already formidable mortality of this class of operations—operations, be it remembered, not by any means entitled to rank with ovariotomy for tumour, because tumour always kills if not removed, whereas a large proportion of these women would certainly not die, and would most of them get well at the menopause. In saying this he was far from wishing it to be understood that he did not approve of these operations of expediency. There are many cases in which the patient is so miserable that he thought she was quite entitled to run the risk of operation. He would now follow the arrangement of Dr. Duncan's paper. The first thing which struck him was that, according to all ordinary ideas, Case 6 in the table, upon which the author had enlarged in his remarks, was not a case of chronic disease of the uterine appendages, but an ordinary ovariotomy for dermoid ovarian tumour. Then he should like to ask Dr. Duncan whether either Case 9 or Case 24 was the one which he saw with him in consultation, and suggested the use of white mixture, with, as Dr. Duncan afterwards told him, the happiest results. Passing to the pathology, he should be inclined to reverse the order of events, as described by Dr. Duncan, in the formation of cystic tube, either hydro-, hæmato-, or pyo-salpinx. He thought it was much more probable that the narrow orifice towards the uterus first became occluded, at first merely by inflammatory swelling, and then by adhesion of the sides of the tube thus pressed together, and that the result of this obstruction was the escape of morbid material backwards, at the open peritoneal end, which caused adhesive peritonitis, an effort of nature to prevent more general contamination of the peritoneum, and hence cystic dilatation of the closed tube. Then with regard to cirrhosis of the ovary being the result of pressure from adhesions, he doubted the conclusions; they knew that cirrhosis did result from inflammatory changes in the course of some or all of the acute exanthemata, and they also knew that such cirrhotic ovaries, performing their functions badly or not at all, were a cause of great suffering, and yet were often found entirely free from adhesion to neighbouring parts. Such ovaries may of course he surrounded by adhesions, but in the great majority of the cases of what may be called adhesion tumours, the ovaries are found large and œdematous, their tunic markedly thickened, their stroma also thickened, and full of abnormal cell growth with much fluid, and their follicles unruptured and tending to become cystic. It is of course possible that such ovaries may, as the result of long pressure from surrounding adhesions, eventually waste and resemble the true cirrhotic ovary; but he thought the two conditions should be carefully distinguished, and that careful inquiry into the history of the patient would leave little doubt in most cases that she had cirrhosis of her ovaries, before she had the pelvic peritonitis. Such patients were especially liable to find their way into the hands of those too active gynæcologists to whom Professor Sinclair had referred, and as a result of treatment, have added to their miseries inflammation and matting of parts around their already diseased ovaries. So, again, he thought the majority of the cases which Dr. Duncan described as congenitally ill-developed ovaries were really not congenital, but the result of acute disease during childhood, especially about the time of the onset of menstruation, in the case which he gave as an illustration, he mentioned, as if of no consequence, the ordinary diseases of childhood; and he (the President) thought these were often of the first

consequence, and were too often overlooked in considering abnormalities in the future menstrual history. In such cases he should carefully avoid the free dilatation of the cervical canal, which Dr. Duncan recommended, because it was just this sort of interference which was likely to lead to inflammatory mischief and adhesions. Then Dr. Duncan said the symptoms of diseased appendages were essentially two—pain and menorrhagia; in his (the President's) experience they were essentially three, for amenorrhæa was even a more important symptom than menorrhagia ; many of the cases which suffer from pain and menorrhagia will cure without surgery. None of the cases, so far as his experience went, which suffer from pain and amenorrhæa, will so cure, and still worse, it was very doubtful if they will cure with complete removal of the appendages. He, at any rate, had found them not always, but very often, most unsatisfactory as to their condition after operation. Then Dr. Duncan said the pain was referred to the side most affected, and left the statement without qualification ; now one of the earliest observations he (the President) made after commencing to operate upon these cases was that the side in which the pain was chiefly complained of was the one in which, so far as one could judge on opening the abdomen, there was least disease, and this he had repeatedly pointed out to those present at his operations. Then again, his cases did not bear out Dr. Duncan's statement with regard to increase of flow being most marked when there was merely salpingitis or ovaritis present, and that when the tubes were distended with fluid this symptom was not marked. He (the President) found that in most of the cases that had had palpable tumour, there had been very marked increase of flow, and often also great irregularity, whereas it was by no means a constant symptom in the milder cases which had not progressed beyond chronic inflammation of the appendages. He quite agreed with Dr. Duncan that rectal examination was very valuable ; indeed, it was often more so than vaginal, and will, as he said, frequently decide whether there was mobility or not, when mere vaginal examination will not do so. He found that with the index finger in the vagina, and the second finger in the rectum, one can often define most accurately the size, relation, and amount of fixity of a swelling in a way which is altogether impossible by any other means. He believed that anæsthesia was rarely, very rarely, necessary, and it should be avoided for several reasons : 1st. Its own inherent risk ; it was in small matters, such as this, that the accidents of anæsthesia often happened ; therefore, avoid it whenever possible. 2nd. When pain was abolished one was very apt to push one's examination to dangerous lengths, and fresh escape of morbid material into the peritoneum might result. He had seen a lady black and blue after an examination under anæsthesia, made by very distinguished gynæcologists. 3rd. The absence of the patient's sensations was a great loss to the examiner, in forming a correct opinion as to the need for operation. Therefore, he would strongly urge the advisability of avoiding anæsthesia for pelvic examination whenever possible. He did not think he used it twice a year, and he doubted if he had ever gained any marked advantage from it ; but sometimes it might be necessary in a very neurotic patient to distinguish between the real and the ideal. They should be very careful in accepting a patient's statement as to gushes of fluid, and unless they knew their man well they should not even be satisfied with the statement of the medical attendant on this point ; the escape of fluid which frequently followed any manipulation of the uterus, and especially the use of the sound, he had seen frequently mistaken for

such gushes. He did not deny the occasional presence of intermittent hydro- or pyo-salpinx; it was a proved fact, but it was a very rare condition. Dr. Duncan said it must not be forgotten that the full benefits of the operation were not usually felt for several months after, but he would say, were never felt till several months after and often not for some years. With regard to treatment, he, in the main, agreed with Dr. Duncan's first and second propositions, but not with the last part of No. 2, where he dealt with "suspected tubal gestation." He (the President) thought there were many cases in which waiting and watching was the only proper course, and he knew of several cases in which many competent observers were agreed as to the probable presence of tubal pregnancy, in which the patients got perfectly well with rest and care without operation. Of course they must always be prepared to operate at once if any urgent symptoms arose. With regard to proposition 3, he found that ergot, given in bitter tonic mixture, for a long time between the periods, in combination with efficient hot douching, was very valuable; but both these things must be properly carried out. He had recently seen a case in which, after a month of very imperfect trial, it was decided that this treatment would do no good. This was a farce. Blistering and tampons he never saw do any good, and the latter frequently did much harm. He did not quite see how proposition 4 was meant to be taken. He entirely disagreed with No. 5 as to dilatation, electricity, and in great measure as to the necessity for operation, or, perhaps, he should rather say the good of it. And with regard to unsexing, he again affirmed, as he had often done before, that removal of both ovaries in a woman, who had no supernumerary ovary, did unsex her, and destroyed the sexual desire and pleasure. He affirmed this as the result of careful inquiry, addressed some years ago to all the patients on whom he had performed the double operation, either in ordinary ovariotomy or in the operations under consideration. He should fully agree with No. 6, if he were sure that it could be decided by mere inspection when an ovary was healthy and when diseased. He was not at all sure that they could always do so, even by careful microscopic examination. He (the President) had only once failed to complete this operation, and that was in the case of a young married lady who had had general peritonitis in her girlhood, and when the peritoneum was opened the intestines were so closely matted and so completely covered in all the pelvic organs that he could not find any of them. If they could reach an ovary or tube or the uterus as guide, he did not think any operation need be left incomplete; it was only a question of time and care, but with the greatest care fæcal fistula will sometimes result, either immediately or some days after operation, from sloughing of a portion of gut damaged at the time of operation. He did not believe that it was necessary to tie in three portions, nor did he think it advisable. The middle loop is very liable to be left looser than one thinks. He thought it was of the first importance to make the trans-fixion through the utero-ovarian ligament; it gave firmer hold, bled but little, and avoided the risk of puncture of a vessel, which was undoubtedly a not uncommon cause of phlebitis. He was more and more convinced, too, of the great importance of using fine silk, and tying slowly and care-fully, by a series of gentle tractions, with a rest between each; if this plan were followed, fine silk was sufficient for most of these cases; he generally used No. 2 now, and he never saw the after-suppurations and fistulous tracks referred to by Mr. Doran. With regard to drainage, he now drained more often than formerly, but left the tube in a much

shorter time—rarely more than forty-eight hours ; it had, however, the disadvantage of leaving, in many cases, a weak place in the scar. He was glad to find that someone besides himself thought flushing was a proceeding of risk. He had tried it in a few cases, but had seen but little good from it, and very decided evil in at least one case, in which it washed pus into the upper part of peritoneum, and the patient died of suppurative peritonitis, clearly commencing from this infection. With regard to the dressing, he was surprised to hear anyone advocating oil silk ; anything that keeps a wound moist was, in his opinion, bad ; and since he had discarded oil silk many years ago, and used simply gauze next the wound, he did not think he had had half-a-dozen cases of stitch hole suppuration in his whole practice. In the main he agreed with Dr. Duncan's remarks on peritonitis and flatulent distension and sickness ; but he was not sure about re-opening in peritonitis. He had seen so many cases get well without it, and he had seen so much evil from interference during the acute stage of peritonitis, that he was inclined to think that, on any large series of cases, opium and atropine and the expectant treatment would beat the heroic. Passing now to Mr. Doran's paper, he had no experience of massage for the cases under consideration, but he should think it was not likely to be of service, and would be decidedly dangerous in a great proportion of the cases. He agreed with what Mr. Doran said with regard to electricity, that it must be a robust faith, indeed, which can believe it capable of opening up closed tubes, destroying adhesions, and killing gonococci, and yet be harmless to the individual in whom it works these wonders. Catheterism of the tubes he should prefer to leave to more skilful and experienced manipulators than himself, and with all their skill he should venture to prophesy that their death rate would be considered greater than that of the skilled and careful abdominal surgeon. Vaginal puncture had nothing to recommend it, and much to contra-indicate its use. With regard to the new departures suggested in abdominal section, he should distrust the permanence of cure in drainage of a hydro-salpinx, and should consider it likely to be far more dangerous than salpingostomy. He thought, in carefully selected cases, there was something to be said for mere separation of adhesions, and he had himself used it successfully for one ovary and tube, where the other clearly needed removal ; but, as he had already pointed out, the difficulty was to distinguish in these cases when an ovary was still healthy. He must most emphatically dissent from Mr. Doran's dictum, that in every case in which mere separation of adhesions was performed drainage and flushing were necessary. The latter he should think rarely, if ever, necessary in such a case, and many cases need not require drainage if hæmorrhage was carefully checked before the abdomen was closed. The presence of the tube was very likely to excite fresh adhesion of the recently freed surfaces. He doubted if mere rest ever cures, and he quite agreed with what Mr. Doran said as to the danger of encouraging chronic invalidism. Rest just before, during, and just after the period was essential, but the patient should be encouraged to take gentle walking exercise at all other times, and in every way to lead a natural life. The curette might be useful in endometritis, before the tubes and ovaries become involved ; afterwards it could not remove the whole of the diseased surface, and the manipulations which it required must be very dangerous in spreading mischief. But in endometritis they were not treating diseased appendages, though they might possibly prevent their becoming diseased if they curetted early enough and with thorough

asepsis. The curette was more likely to set up disease in the appendages than to cure it. He had had practically no experience of the mental conditions as after-effects to which Mr. Doran referred, and he could not believe that they were at all common. He had already referred to what Mr. Doran said as to persistent fistulous tracks; bad ligaturing, or too thick silk, sepsis, and a too prolonged use of the drainage tube must, he thought, be at the bottom of such misfortunes. A disappointing continuance of pain they certainly had in some cases, and he thought it usually arose from inflammatory troubles following the operation and consequent adhesions, especially of the intestines, and he expected was commonly due to imperfect asepsis. He should expect the artificial opening made in salpingostomy to be closed in twenty-four hours after the operation, by adhesion to some neighbouring peritoneal surface in about nine-tenths of any series of cases in which it was made. It was a somewhat suggestive circumstance that neither Mr. Doran nor Dr. Duncan had referred to the persistence of menstruation after these operations. He had always believed, and still firmly believed, that when it did occur it was due either to imperfect removal of ovarian tumour or to the accidental presence of a supernumerary organ. His own experience of it was practically *nil*. Neither did they offer any remarks on the relative value of removal of tubes and ovaries as affecting the menstrual flow. He still had no doubt that if all the ovarian tissue a woman possessed was cleanly removed she ceased to menstruate, though she might, for a time, have periodic metrostaxes, the result of long habit, or through irritation of the ovarian nerves by the encircling ligatures and the changes going on around them. In the last discussion at this Society, in 1888, Mr. Tait quoted, with approval, the following words of Dr. Mary Dixon, in speaking of the operation for removal of the appendages :—"It will save more lives than ovariotomy, because more need it." He did not know that he had ever read a more absurd or misleading statement. The diseases for which ovariotomy was performed always kill in time without operation; the diseases for which they removed the appendages rarely killed, and in the vast majority of cases did not even shorten life, though they often rendered it very wretched. How then could they compare the operations as to their life-saving? He was strongly of opinion that the views held and promulgated by many authorities as to the amount of sterility due to these chronic diseases of the appendages were exaggerated and misleading. He could quote some striking cases in support of his view, if time permitted it. He was also of opinion that both ovaries and tubes were too often removed when one side was only really diseased, and that many of the cases in which pain recurred on the other side were neurotic cases, which should never have been touched at all, and that most of the rest were the result of bad surgery at the first operation, want of thorough cleanliness or asepsis, and consequent inflammatory trouble.

Dr. Routh was somewhat surprised to find that mutilations in the past appeared to be more objected to than in the present day. Mr. Brown's clitoridectomy cases led to his condemnation by the entire profession, and yet, as evidence proved, neither sexual faculty nor mental trouble resulted therefrom. On the contrary, women became mothers who had been barren before, and the sexual orgasm was even intensified. Witness the Abyssinian Jewesses, who were in part circumcised, that is, clitoridectomised. The Abyssinian men often became Jews to marry these women. Nor was the deformity apparent after years. The castra-

tion of a woman was a lasting mutilation, and made a woman often most miserable. He did not say it should *not* be performed in cases of life and death, and as a *pis aller*, but not otherwise. In these thirty cases of Dr. Duncan we were told some had been ill for years, but we were not told what treatment had been followed before ; and he was sure if proper active treatment had been pursued, they would not probably, at least several of them, have got in the state they were when treated by Dr. Duncan. The President had found fault with many of the cases, and Mr. Doran had told them that in many the mere separation of the adhesions would quiet the patient and cure them. He also had protested against necessarily taking away the appendages, because the abdomen was opened to see what the mischief was. Mr. Doran had also spoken of salpingostomy as recommended by two gentlemen abroad, and successfully practised, in cases of *hydro-salpinx*. He had not referred to Dr. McCan's three cases, also successful. Why remove, therefore, except in very exceptional cases, the appendages in hydro-salpinx. The President had spoken not encouragingly of puncture *per vaginam* or *rectum* into a pyo-salpinx or ovarian abscess. Now, in some of these cases the pus contained was intensely fœtid, full of adhesions, and the sac very thin. In all abdominal sections there was the greatest risk of rupture. But if an aspirator puncture was made through the vagina or rectum, and, after voiding it it was washed out with iodine and a drainage tube kept in, and the sac daily washed out with an antiseptic, it would contract, and recovery follow. He had once seen this bursting take place in an abdominal section, and death followed ; while he had had several cases in which the aspirator and subsequent aseptic washings were carried on, and perfect cure followed, except in one case, which had left him, and the ultimate result of which he knew not. Now, he did not accuse Dr. Duncan of immorality in these operations, because he had operated well, and he was sure he acted to the best of his judgment ; still, if it could be proved that the comfort and life of the patient could be ensured without castration he thought that every moral or Christian man would not practise it There was another point he wished to dwell upon. Dr. Duncan had said he had not included *all* his cases, because the results of some of those practised in Waterloo Road Hospital were so unfortunate. But still, in the abstract published, there were five successful cases treated in that hospital. Now, this would prove one of two things. Either that at first he was not successful, from want of experience or the unhealthy state of the hospital, but that, later on, both his skill and the sanitary state of the hospital improved. Thus we should be able to estimate the full value of the cases, because then we should have the whole truth declared. Lastly, it had been said that in pyo-salpinx electricity was never to be employed. Now, whoever said it should? What he did know was this : in cases of early gonorrhœa (three or four days) with urethritis, you often felt the appendages swollen and tender. Now, if in those cases you applied a negative current within the uterus, it acted in some way or other, perhaps as a derivative, and recovery followed rapidly. This he had seen done by Apostoli, but no man in his common senses would think of applying electricity in an advanced case of pyo-salpinx.

Dr. Heywood Smith said, in answer to the remarks that had fallen from the President, that in his experience menorrhagia was a more frequent symptom in cases of chronic disease of the uterine appendages than amenorrhœa ; but he agreed with the President in his observation that the pain was often on the side opposite to the diseased ovary, and he

thought the explanation was that the swollen ovary pushed the uterus over to the opposite side, and the pain arose from the pressure on the cervix uteri or the ovary on that side. He was surprised to hear the President say that in cases of tubal pregnancy it was better to wait, and that such cases often got well. He (Dr. Smith) considered it endangered the woman's life to wait beyond the third month of utero-gestation, as, though the pregnancy might become extra-tubal, and so abdominal, she ran the risk of rupture. He would like to hear from the President his explanation of such a statement. With regard to what had been said, both by the President and Dr. Routh, on the question of the sexual appetite, in his experience it was quite the exception when desire had been at all lessened. In those cases where it was desirable to lessen a morbid sexual craving, the operation of clitoridectomy afforded a far more satisfactory result than the removal of the uterine appendages. It was beside the mark to speak, as Dr. Routh had, of the immorality of the operation, of the unsexing of the woman, and her consequent degradation. Our duty was not only to save life, but lessen misery, and in the case of those who had to earn their daily bread by labour, it was incumbent on medical men, if possible to relieve patients of their suffering. As to the question of salpingectomy alone, he considered it was worse than useless to leave the ovary without its tube to become perhaps the seat of further complication and pain.

Mr. Lawson Tait was glad to find himself in the main in agreement with the author of the paper and with those who had spoken. He disagreed entirely with the moral view of the question which Dr. Routh had put forward. He was surprised to hear the President say that flushing did harm, for he was in the habit of using it most days of the week, and he had never had a mishap from it. The same remarks applied to drainage. Sir Spencer Wells had said that the drainage tube should be done away with, but he found it most valuable, especially in giving warning of the beginning of hæmorrhage. He kept it in a much shorter time than he used to—two or three days, instead of five or six. As to the Staffordshire knot, like other knots, it could not be tied with negligence. Once or twice he had failed with it, through not attending to his own directions for its use. It was not fair to say that the knot was not to be trusted ; it was the man who tied it who was not to be trusted. Referring to the quotation of Dr. Mary Dixon, he called to mind the fact that at the time it was made there was great scepticism as to the very existence of pyo-salpinx, and Dr. Fowler's remarkable figures from the Middlesex Hospital records showed how fatal that disease was. Hence it was not unfair at that time to say that more women would be saved by removal of the appendages than by ovariotomy. Now, however, the fatality of this disease was not so great, because many cases were treated early. He was constantly protesting against the mixing up of cases which ought to be operated upon with those which should not, and because some of his critics were unable to recognise those which should be treated by operation they accused the operators of practising ignorant empiricism. He emphasised the essential difference between cases like those neurotic ones which demanded a guarded opinion and those cases of clear diagnosis in which operation was the only thing to save life, or at any rate to relieve suffering. In cases like those of menstrual epilepsy, where he had once removed the appendages he now hesitated to do so ; but still, from recent experience, he was inclined to think that a vast field lay before them in these neurotic cases where operation might give

relief. The gain at any rate would probably be greater than the evil, moral, Christian, or other, likely to result therefrom. Referring to the treatment of pyo-salpinx by electricity, he said it required a very robust faith to believe that this measure could cause pus to dry up. Let the advocates of it try a whitlow or a suppurative arthritis, where one could see and judge the result, before making assertions which could not be proved.

Dr. Lewers said it was most important to inform ourselves as far as possible as to the natural course taken by cases of diseased Fallopian tubes when not operated on. He referred to a series of seventeen cases of dilated tubes which he had described in a paper read before the Obstetrical Society as bearing on this point. The late Dr. Matthews Duncan had commented on the apparently advanced age of the patients in this series, and had argued that, as fifteen of the seventeen died from causes unconnected with disease of the Fallopian tube, it was probable that in many of the cases the disease had undergone the process of natural cure. Dr. Lewers believed this to be the true explanation. He considered, therefore, that a prolonged course of palliative treatment—one year and a half to two years—should precede operative interference. He commented on a case of ill-developed ovaries and uterus described in Dr. Duncan's paper. He could not agree that because the sound passed only 2 inches in a virgin of 20 the uterus was ill-developed, as this measurement was the normal length of the uterine cavity in the virgin according to all standard authorities. The specimen of the appendages removed in this case had been shown, and he felt bound to say that, as to size and all other obvious characters, they, in his opinion, showed no abnormality.

Dr. Amand Routh emphasised the value of palliative treatment in cases of tubal disease where the only alternatives to relief were chronic invalidism or surgical interference. He had seen many apparently complete recoveries, and instanced a lady whom the President had seen with him five years ago with double tubal disease who was now in perfect health. He noted how averse the surgeons at the Samaritan Hospital were to this operation, and how ready they were to transfer the cases of tubal disease to the medical side of the hospital for a prolonged course of palliative treatment. Rest was essential, especially sexual rest, whilst free purgation, ergot, hot douches, and the treatment of any existing diathesis were very important. He had found that much relief, both to the pain and swelling, was afforded by the occasional application of tin. iodi to the posterior vaginal *cul-de-sac*.

Dr. Rutherfoord was struck by the large number of diseased appendages which Dr. Duncan had found and the very few ovarian cysts. At the Samaritan Free Hospital he had been greatly impressed by the fact that, though more abdominal sections were performed there than at any other London hospital, the number of ovarian tumours exceeded by a very large majority the number of diseased appendages removed. He could not account for this difference between Dr. Duncan's practice and that which obtained at the Samaritan. Perhaps Dr. Duncan could give some explanation. The ovaries exhibited by Dr. Duncan as congenitally ill-developed were, in his opinion, judging from their size, contour, consistence, and general appearance, perfectly healthy and normal ovaries. He could not understand Dr. Duncan's method of treating cases of dysmenorrhœa by dilatation of the cervical canal when the uterus and ovaries were congenital, especially as in the case recorded by Dr. Duncan the dysmenorrhœa was clearly ovarian and not uterine. He regarded the statement

that no glands existed in the Fallopian tubes as bold, and referred Dr. Duncan to a paper by Mr. Sutton, in the 'Obstetrical Transactions' for 1888, and to Ballantyne's and Williams' extensive researches published in the 'British Medical Journal,' in January, 1891.

Dr. CULLINGWORTH said that he was in general agreement with the views and practice advocated in Dr. W. Duncan's paper. In some minor details, no doubt, the paper was open to criticism, but previous speakers had already alluded to most of the points to which he had intended to take exception. He thought it was a mistake to include a case of suppurating dermoid cyst of the ovary in a series like the present, and, in all probability, the reader of the paper, now that he had heard the matter discussed, thought so himself. There was a point bearing on diagnosis to which Dr. W. Duncan had scarcely called sufficient attention, and that was the great significance of recurrent pelvic peritonitis as an indication of the probable presence of chronic disease of the uterine appendages and especially of the Fallopian tubes. It was true that recurrent pelvic peritonitis was occasionally caused by other conditions, as, for example, disease of the vermiform appendix, but, in the vast majority of cases, occurring in the female, it was due to chronic inflammatory affections of the tube or the ovary or both. The fact, therefore, of a patient being the subject of such recurrent attacks, should always arouse suspicion as to the condition of these organs, and lead to a thorough and careful examination of them. The pith, however, of Dr. W. Duncan's paper lay in the series of propositions on the subject of treatment; to some of these propositions he would, therefore, now address himself. To the first two he fully assented; but, with regard to the course of treatment laid down in the third, he thought that, in omitting all mention of rest, he had left out the one really important element. The treatment laid down by Dr. Duncan had in itself little or no value. It would only succeed if absolute rest were enjoined at the same time, and then it would be the rest that did the good. Tonics, for example, could only have a very indirect influence on pelvic inflammations. Taking the next remedy on the list, it was no doubt important to prevent accumulations in the lower part of the large bowel, but, beyond this, aperients were of no special value. Repeated blisterings he regarded as a cruel and useless barbarism, and the same might be said of the practice of painting the roof of the vagina with iodine, as recommended by one of the previous speakers. Hot vaginal douches were harmless, and no doubt were fashionable, but it could not be seriously maintained that they were capable of exerting any remedial action on parts so distant as the ovary and Fallopian tube. Dr. Emmet, their strongest advocate, adhered to the old view that these were cases of cellulitis. It might be possible for hot douches to have some effect in cases of inflammation of the cellular tissue of the pelvis, so that Dr. Emmet and those who thought with him had some excuse for ordering them and even for regarding them as important elements of treatment. But it was now well known that pelvic cellulitis was a comparatively rare affection, and that cases formerly classed under that head were for the most part cases of peritonitis secondary to salpingitis, affections that hot vaginal douches could not influence. The only remaining remedy in Dr. Duncan's list was glycerine tampons. The most enthusiastic admirers of these uncomfortable applications did not credit them with any power beyond that of draining the tissues in immediate contact with them of a little watery fluid. What possible influence for good could they have in chronic inflammatory disease of the uterine appendages?

Probably neither they nor the douches did any harm, however, and if their employment enabled the treatment by rest to be more readily accepted by the patient they fulfilled a useful function. Nevertheless, he must repeat that it was the rest did the good, and the improvement that rest brought about was often most marvellous. It permitted absorption of peritoneal exudations, it gave freedom from pain, and it restored patients to a condition of apparently good health. If, however, the Fallopian tubes contained pus, the mischief would again sooner or later declare itself and it would become evident that the improvement had been merely temporary. On this ground he deprecated unnecessary waiting before operation, especially in the case of women belonging to the labouring class. With Dr. Duncan's sixth proposition he quite agreed in principle. There was, however, one case in which his own experience had shown that it was not always safe to leave parts behind because their external appearance was normal. He alluded to the case where there was a pyo-salpinx on our side and an apparently normal tube on the other. He had two or three times found such apparently healthy tubes to contain pus, and he believed that it would prove to be a wise rule to remove both tubes in all cases of pyo-salpinx. He heartily assented to the last proposition as to the unjustifiability, in the present state of our knowledge, of removing the uterine appendages in cases of neurosis, in any case, in short, where, on physical examination, no obvious lesion could be detected. He thought that, on this point, Dr. Duncan's practice scarcely agreed with his preaching. The removal of ill-developed ovaries came very nearly if not quite in the same category. On the subject of drainage, and flushing the peritoneal cavity, he had nothing to add to Mr. Lawson Tait's remarks, with which he entirely agreed. The President had anticipated him in some remarks he had intended to make on the various methods of treatment mentioned in Mr. Doran's paper. There was only one point in that paper to which he would, therefore, refer. Mr. Doran spoke of the difficulty of estimating by statistics the value of treatment by rest. He desired to point out that, even if we were able to obtain such statistics, they would be liable to a fallacy that would render them utterly valueless. The length of time during which collections of pus in the pelvis may lie dormant in patients who have been treated by rest is almost incredible. In a valuable paper by Dr. R. B. Maury in the current number of the 'American Journal of Obstetrics,' which he commended to the attention of some of Dr. Duncan's critics, there were two signal examples of this prolonged immunity in patients with pyo-salpinx, not only from active pelvic mischief, but from any indication of ill-health. In one case the period of latency extended over four years, in the other it actually extended over seven years. Such cases might easily be reported as instances of permanent cure. Yet in both the disease ultimately asserted itself and called for energetic and radical treatment. He had notes of similar cases, though not extending over quite so long a time, in his own practice at St. Thomas's Hospital. It had been asked why these cases are not met with more frequently in the post-mortem room. The answer was that they were not looked for. When Dr. Kingston Fowler and Dr. Lewers instituted a systematic examination into the condition of the female pelvic organs in the post-mortem room, they found them seriously diseased in a proportion that was quite startling. If such a systematic examination were the rule instead of being the exception, there would be no lack of evidence to show that chronic inflammatory disease of the uterine appendages is not only a

common affection, but one that not infrequently ends fatally. It was time that our pathologists realised that the parts below the brim of the pelvis were of some importance to the female economy, and were worth examining in the ordinary routine of the post-mortem room.

Dr. Duncan, in reply, was very glad to find that in the main the various speakers agreed with the views expressed in the paper. He said the thirty cases tabulated began some two years ago, from the date when the obstetric physicians at the Middlesex Hospital obtained the right to perform abdominal section. Prior to that date he had removed diseased appendages nine times, with one death from septic peritonitis. Since the last published cases he had operated ten times, with, unfortunately, two deaths, the first from slipping of the pedicle ligature and fatal hæmorrhage, and here he emphasised the importance of tying the broad ligament in three places. He could not agree with the President that doing so was dangerous, and he meant in future, in those cases where the broad ligaments were bound down by old inflammation, to follow out this method, and trusted by doing so to eliminate one cause of death. The last death occurred a week ago, owing to a bit of the omentum getting strangulated, first in the drainage tube, and then in the abdominal wound after the tube was removed. He still maintained that whenever tubal gestation was suspected an immediate operation was called for. He could not agree that dilatation of the cervix was attended by risk, provided the strictest antiseptic precautions were taken. He had never had any ill-result. His experience was similar to that of Dr. Heywood Smith, who said that amenorrhœa was the exception and menorrhagia the rule in cases of diseased appendages. He was sorry to hear Dr. Routh advocating puncturing a pyo-salpinx *per vaginam*, as it was a most dangerous proceeding. He mentioned a case on which he intended performing abdominal section the following morning where a pyo-salpinx had been punctured two years ago, causing general peritonitis, and not curing the disease. He did not think the operation of making an artificial ostium was of practical use, and he had no intention of performing it. He was sorry to hear Dr. Lewers recommending that these cases should be treated by an expectant treatment for eighteen months. Indeed, he was in complete accord with the forcible remarks of Dr. Cullingworth, who insisted that the time mentioned in the paper (two months) was too long. He also quite agreed with the last speaker as to the frequency of recurrent attacks of pelvic peritonitis in the causation of chronic disease of the uterine appendages.

Mr. Alban Doran regretted that the lateness of the evening prevented him from replying fully to some of the observations which had been made on his paper. He defended the practice of setting free tubes and ovaries by breaking down adhesions, without removal of those organs. It by no means followed that, because there were adhesions before the operation, more would necessarily form afterwards. The patient was surrounded by more or less unfavourable influences before, which favoured the development of adhesions. After the operation the rest in bed and general care tended to prevent even the most local form of adhesive peritonitis. He was not at all sure that adhesions formed around the drainage tube, or that an artificial ostium, cut out in the operation of salpingostomy, must necessarily be closed by lymph soon after the patient was put back to bed. Mr. Doran always believed that tubal diseases arose from infection passing up the vagina and uterus, but this fact did not justify the use of the curette to the endometrium when the tube was

already extensively diseased. He had purposely avoided all references to sexual appetite, in his own contribution, as it was exceedingly difficult to get sound and trustworthy evidence on an entirely subjective condition which involved innumerable matters of delicacy. Lastly, in comparing ovariotomy, the practical basis of abdominal section, with removal of the appendages, we must act, Mr. Doran maintained, like Danton, in cases of ovarian cysts, and like Fabius Cunctator, in chronic disease of the appendages. When we have to deal with an ovarian cyst, we must avoid all Fabian tactics, all delay. We must be bold and operate at once, we must be bold and not get frightened after opening the abdominal walls, and lastly, *encore de l'audace*, as Danton said, we must be bold and patiently grub up the base of the cyst out of the deepest pelvic connections, if need be. With chronic disease of the appendages, the opposite principle was our duty. The policy of Fabius, masterly intelligent delay, would save many ovaries ; on the other hand, the name of Danton was significant in this respect. The sacrifice of many heads, unjustifiable even after his own views of fraternity, suggested an obvious parallel.

February 9th, 23rd, and March 9th, 1891.

ANGINA PECTORIS, ITS NATURE AND TREATMENT.

By R. Douglas Powell, M.D., F.R.C.P.

In response to the courteous and flattering invitation of our Honorary Secretary, I elected to bring forward for discussion to-night the subject of *angina pectoris*, and for two chief reasons : first, because I wanted to know more about it myself, and to glean the opinions of my working fellow-physicians on the matter; secondly, because there are many questions of physiological and practical clinical interest about angina which require further elucidation, and which, if we can only succeed in definitely formulating them, will in time be answered, and with more exact knowledge our hands will be strengthened in the treatment of this always distressing and sometimes deadly malady.

Much consideration and some experience and reading on the matter have convinced me that angina is not a simple disease, as it has been supposed by some authors, with heart anguish and syncope for its symptoms, and coronary obstruction and fatty change for its pathology, but that, like asthma, it has a far broader neuro-pathological foundation ; that it is a malady in which the characteristic symptoms may be observed to range between the

wide limits of remediable functional disorder and fatal organic lesion of the cardio-vascular system.

Let me point my further remarks with the help of a very few cases which I will relate.

Miss D——, aged 45, a delicate and decidedly neurotic lady, rather given to self-examination as regards symptoms, and to the view that as much can be accomplished by faith-healing as by grosser remedial agents, consulted me July 5, 1890, on account of having felt the heart's action disturbed during the past four or five years. She does not have distinct attacks of palpitation, but is constantly aware of the rather forcible shock of the heart's action, and has noticed some irregularity of beat, and has been conscious of occasional pauses.

Many years ago she was delicate at the chest and suffered slight hæmoptysis. Fifteen years ago she had rheumatic fever, and she has at times had rheumatic pains and swellings in one or more joints. Ten years ago (she is now 45) this lady would occasionally, when riding hard across country for 6 or 7 miles at a stretch, be seized with severe pain at the heart, lasting two or three minutes and interfering with breathing. She would not stop for this pain, although for the time severe, and it would pass away whilst still riding fast. She now gets occasional attacks of sharp pain similar to those which occurred during violent riding exercise; at other times the pain is more prolonged, slow, and dull. She has occasional giddy feelings, and feels very ill but never faints. Occasional numbness of arms and great chilliness are experienced, relieved only by sharp exercise or extra clothing. She can often walk quickly without distress; at other times quick movement brings on an attack.

The catamenia are quite regular and are attended with no special suffering. There are no distinct flushings or perspirations.

Digestive functions well performed, and nutrition fairly maintained. The pulse is habitually rather quick and of high tension.

On July 3 the heart was noted as being slightly enlarged, and the first sound at the apex not clear.

On January 22, 1891, I was consulted a second time, and found the pulse small, cord-like, and of high tension of a peculiar kind, *i.e.*, on using hard pressure with one finger the pulsations were almost if not quite as well felt by the second finger placed further along the vessel, Fig. 1, *b*. On exercising gentle pressure with the second finger the pulsation was, however, observed to be caused by a back-wave from the periphery, due, in fact, to a reverse current, and on compressing with this finger firmly the pulse was absolutely stopped between the two fingers, Fig. 2, *b*, *c*.

Heart's apex-beat barely within nipple line, and impulse increased in force and somewhat labouring. Intermittence about twice in the minute. First sound muffled, but no distinct murmur.

Urine (July, 1890) acid, pale yellow, 1027, no albumen or phosphates, no sugar, a few oxalate of lime crystals; (December 3) clear, 1020, and no albumen or sugar.

No distinct gouty inheritance. Father an active sportsman. *Mother died almost suddenly of heart failure*, without any preceding illness of a definite kind.

Angina pectoris has been variously defined. Heberden,* who

* Heberden, 1772, 'Med. Trans. Coll. Physicians, London,' vol. ii, p. 59.

first drew attention to the disease more than a hundred years ago, gives indeed no definition; but the graphic account of the disease given by that great physician is a model of descriptive accuracy and lucid exposition, which, when read in the light thrown upon it by recent observations on the physiological mechanism of the cardio-vascular system, remains true, although with an extended meaning. Heberden, however, although he did not give any formal definition of the disease, expressed a very definite opinion as to its nature "as pertaining to distension and not at all to inflammation," his term "distension" being in Dr. Latham's* opinion equivalent to spasm, and "inflammation" to organic disease. Dr. Latham's definition of angina is, "Pain of extreme severity passing through the chest from the sternum to the spine, arising suddenly and ceasing suddenly, and accompanied while it lasts with a feeling of impending death" (p. 448). In commenting on Heberden's views of angina, Dr. Latham makes the shrewd and true remark, "Grant that it is properly and essentially spasm, spasm does not necessarily exclude organic disease. It may be and often is altogether independent of organic disease; but organic disease may be and often is instrumental in producing it."

Walshe understands "by the terms angina pectoris, syncope anginosa, and suffocative breast pang, a paroxysmal neurosis, in which the heart is essentially concerned."

Sir Richard Quain gives a more adequate definition when he says, "Angina is an affection of the chest characterised by severe pain, faintness, and anxiety, occurring in paroxysms; connected with disorders of the pneumogastric and sympathetic nerves and their branches; and frequently associated with organic disease of the heart."

The facts about the patient whose case I have related and which justify it being included under angina are (1) that on or during exertion she is sometimes seized with decided præcordial pain and oppression (and it is to be observed that the attacks are more frequent and more easily provoked now than years ago); (2) she has a pulse of high tension; (3) her heart is somewhat enlarged, but without other lesion; (4) she is of decidedly nervous temperament.

I would regard her case as one of the first degree or vaso-motory variety of angina, in which the neuro-vascular system is

* Latham, 'Diseases of the Heart,' 1845, p. 460.

unduly sensitive, so that the arterioles under slight extra stimuli—exercise, chill, exertion—contract, suddenly raising the blood pressure, which habitually ranges high, and causing the heart to labour painfully. It is to be observed in this case, and the symptom is a positive characteristic of its vaso-motor character, that the same cause, *e.g.*, exercise, will not always bring on an attack, and that in an earlier stage of her malady the pain would pass off during a continuance of the exertion. In some cases of angina pectoris the patient cannot remain still, but gets up and walks about the room in great uneasiness (Nothnagel). This kind of ambulatory uneasiness is rather characteristic of the vaso-motory variety of the disease. In *angina pectoris gravior* the patient is almost always compelled to adopt some rigid position, often standing or leaning forward with the arms and chest fixed.

The vaso-motor nervous system in reference to angina was of course unknown in Heberden's and even in Latham's day, and has only been practically taken note of in this connection by modern physicians, especially by Nothnagel,* Broadbent,† Lauder Brunton,‡ and Eulenberg.§

The case I have above alluded to is a comparatively slight one; some might regard it as one of habitual high tension of the circulation with slightly paroxysmal increase of such tension. Such a view would content me the more as I wish to maintain the close affinity between the two conditions. The high-tension pulse is familiar enough to all of us nowadays; its conditions are admirably set forth by Dr. Broadbent in his work on 'The Pulse.'

There is one point, however, respecting the pulse of high arterial tension to which I would wish to draw attention, and which was illustrated by the first case brought forward. In cases of high arterial tension in albuminuria, in gouty conditions, and in vaso-motor neurosis, one often hears the pulse spoken of as being *incompressible;* press as hard as you may with one finger, you feel the pulse almost as distinct as before with the second finger, Fig. 1, *b*. If, however, the second finger be pressed attentively down upon the vessel it will be found that the pulsation felt is a reflex wave from the periphery, Fig. 2, *b*, and if this finger be pressed firmly

* 'Deutsches Archiv,' iii, 309, 1867.
† 'The Pulse,' p. 152.
‡ 'Lancet,' ii, 97, 1867.
§ 'Ziemssen's Handbuch,' xii, 2, 1875.

down and a third finger placed between the other two, Fig. 2, *c*, the pulsation of the artery is no longer felt. To produce this phenomenon, it is obvious that the peripheral resistance in the minute vessels and capillaries must be very great, so that when the vessel is compressed at any point its continuity remains not only full but receives an impulse backwards from anastomotic vessels and capillaries beyond. It is a misnomer to call it an incompressible pulse. I have ventured to speak of it as high-tension pulse with peripheral reflux.*

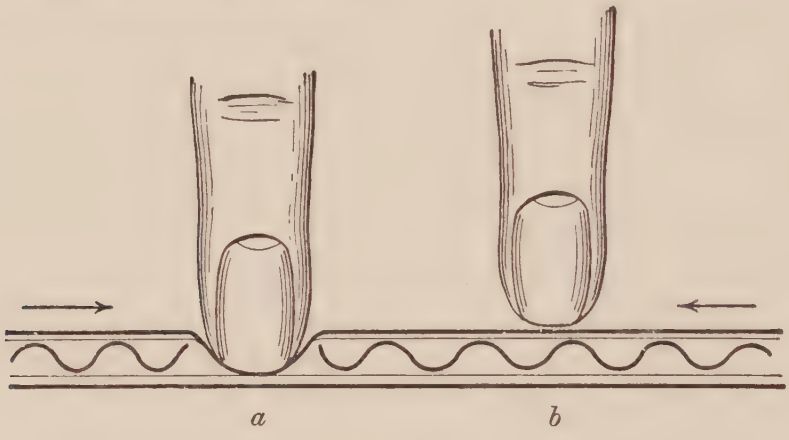


Fig. 1 represents continuity of radial artery; with the finger pressing upon the pulse at *a*, the second finger touching the vessel beyond still feels pulsation. This is the condition recognised as "*incompressible pulse*."

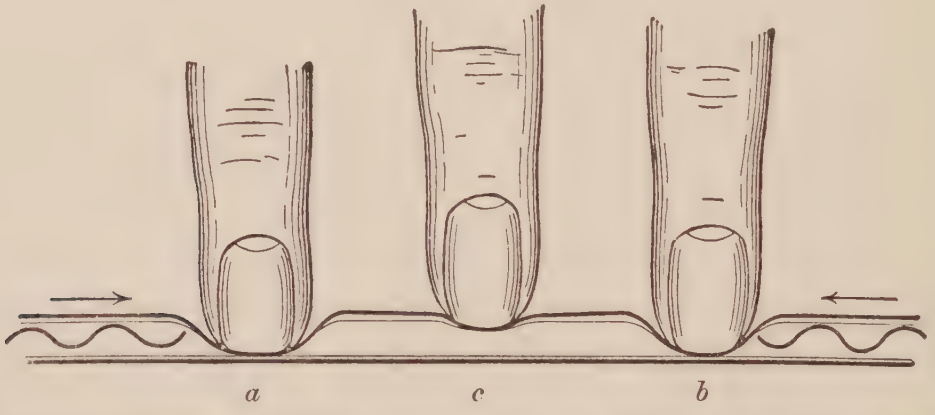


Fig 2.—The finger still firmly compressing the artery at *a*, a second finger pressed down at *b* perceives the pulse still beating, but the impulse may be noted to come *from* the periphery. A third finger placed between the other two at *c* finds all pulsation arrested.

The influence of habitually increased arterial tension in producing at first functional disturbance and ultimately organic

* 'Lancet,' Sept. 12, 1888, "Clinical Note on High-Tension Pulse with Peripheral Reflux in Albuminuria."

lesions of heart and vessels is great, and the results are widespread and disastrous. It is, I believe, the essential pathology of the majority of cases of angina, as bronchial hyperæsthesia is of asthma. The mechanism of vaso-motory angina is paroxysmally increased blood pressure from spasm of systemic vessels. Might we call it an asthma of the vascular system? The heart is the suffering organ. Does it ever succumb in the attack? The suffering would appear to be at least as great in certain pure vaso-motory as in graver cases, and in some I believe the result to be fatal without any heart lesion.

A medical man some years ago consulted me on account of some severe paroxysms of breast pang. He was about 30 years old, and had recently lost his wife from an acute illness. A few weeks afterwards he was early one morning subjected to great annoyance and shock on finding that some flowers placed on his wife's grave had been scattered about. He went home, and whilst in the act of shaving he was seized with intense præcordial pain and oppression, having all the characters of angina. On several mornings afterwards, whilst similarly engaged, he was taken with similarly acute suffering. On most careful examination I could find no evidence of enlargement or disease about the heart. I cannot recall this gentleman's name, and so cannot find my note as regards the pulse; but I regarded his case as one of vaso-motory angina, and recommended him to sell his country practice, which was in a lonely part, to travel for six months, and then to resume work in a city suburb. He did this, and when I saw him a year or two afterwards he had had no recurrence, and he appeared in all respects sound.

Mr. B——, a hale-looking gentleman of 68, of active habits and long residence in Australia, had passed uric acid crystals with suffering on one or two occasions, and had also had eczema. A month before he consulted me he was seized with sudden præcordial pain whilst going up stairs to bed, which obliged him to stop. He had subsequent attacks at night, and occasionally in the day, attended with the usual symptoms of præcordial pain and oppression. He observed after the first attack that he could generally tell when they were coming on by the pallor and "deadness" of his hands, and in one well-marked attack which he had in my consulting room this phenomenon was distinct and accompanied by a small quick and thready pulse, and some lividity of features. The attacks were always attended with flatulent distension of stomach. The heart was somewhat enlarged, its action quick and weak; there were no other morbid conditions to be observed. Under treatment by amyl nitrite for the attack, and small doses of digitalis and arsenic with nitro-glycerine, he held his own, but a marked improvement occurred after an attack of

gouty eczema, and after being two years under observation he returned to Australia in greatly improved health.

I take this to have been a case of vaso-motory angina in a person with a heart a little the worse for wear, gouty contamination of blood being probably the exciting cause.

We are greatly lacking in post-mortem experience in cases of vaso-motory angina; suffering may be great, but death is rare during an attack. The attacks if unrelieved by art come to a natural end, probably through the depressor nerve of the heart, and possibly some analogous mechanism of the small vessels under the stimulus of pressure inhibiting and controlling cardiac and vaso-motor spasm. Cases of angina have been met with in which, the patients having died of something else, no disease of the heart has been found. Moxon* has published one such case. There can, I think, be no doubt that, as pointed out by Dr. Broadbent,† attacks of sometimes fatal cramp whilst bathing or swimming are due to the sudden oppression of heart from contraction of the systemic arterioles, in fact, to vaso-motory angina of an intense kind, and with, perhaps, a fatigued heart. There must be some post-mortem experience of such cases forthcoming, which may, I trust, be elicited in discussion. It is held by physiologists that when the systemic arterioles are contracted by cold the visceral arterioles are dilated, and *vice versâ*, and thus the blood pressure is in a measure equalised. Excessive flow of urine, a symptom of high arterial tension, and common both in vaso-motory and in the graver forms of angina, is a natural means of reducing the intensity of the arterial pressure. It is highly probable, however, that under conditions of severe chill and exertion the whole arterial system may respond, and it would be especially interesting if any evidence were forthcoming as to whether the coronary arteries themselves partake in vaso-motor spasm. Huchard‡ considers tobacco angina, to which he gives the dignity of a separate group, as due to spasm of the coronary arteries. Dr. George Johnson maintains that in a certain stage of cholera the pulmonary arterioles are spasmodically contracted. I have seen two or three cases in which, after each anginal seizure, the patient has shown physical signs—impaired resonance at one base, subcrepitant

* Moxon, 'Lancet,' vol. i, 1881, p. 689.

† Broadbent, 'The Pulse,' p. 152.

‡ Huchard, 'Maladies du Cœur,' 1890.

râle, cough, and blood-stained sputa—of temporary congestion of one lung at the base, suggesting that the right side of the heart had borne the brunt of the attack, and that stasis in the lungs had ensued upon its temporary enfeeblement. I observed this to occur on several occasions at long intervals in an old gentleman, who finally died of cerebral hæmorrhage. The attacks always occurred at night, and were attended with marked anginal pain. The tight state of the arteries in uræmic conditions, and the persistent cardiac distress with which many such cases terminate, we may almost describe as chronic persistent angina; but it is in the so-called cases of cardiac asthma in uræmia that the analogy with vaso-motory angina chiefly holds good. I saw a striking example of this with Dr. Travers, of Kensington, and another with Dr. ——, of Maida Vale, in both which, as indeed in all such cases, marked relief was obtained from nitro-glycerine and its allies. Sewall and Donaldson* have specially drawn attention to the great dependence of the systole as regards its energy and completeness upon the amount of fluid within the heart. Within tolerably low fluid pressures the sinus and auricle become so distended as to be powerless to contract.

I next come to speak of *vaso-motory angina with degenerative cardiac disease*, the so-called true angina—true only, so far as I can gather, because fatal. Professor Gairdner says, "The fact of sudden death, superadded to the evidence of certain sensations preceding death, may be considered to afford the nearest approach we have to an accurate definition of the disease."†

Ever since Jenner first observed that ossification or obstruction of the coronary arteries was commonly found in persons who had died of angina, this coronary obstruction has been generally regarded as giving the pathology of the disease.

Dickinson,‡ Balfour,§ and Greenfield‖ appear to hold this view. Huchard¶ is of opinion that true angina is always the result of an ischæmia of the myocardium, due in a majority of cases to organic

* 'Journal of Physiology,' vol. ii, p. 361.

† Reynolds's 'System of Medicine,' vol. iv, p. 535.

‡ 'Pathol. Trans.,' vol. xvii, "Three Cases of Angina Pectoris depending upon Occlusion of the Mouths of the Coronary Arteries."

§ Balfour, 'Diseases of the Heart,' p. 396.

‖ 'Pathol. Trans.,' vol. xxvi, "Stenosis of Orifice of Coronary Artery due to Endarteritis Deformans of Commencement of the Aorta."

¶ Huchard, *loc. cit.*, p. 437.

or *functional* narrowing of the coronary arteries. Huchard observes that only in cases of tobacco-poisoning does angina occur without organic disease of the coronary arteries, and that then the symptoms are produced by spasmodic contraction of those vessels. He explains the occurrence of sclerosis and stenosis of the coronaries without angina by collateral circulation. Whilst I am quite ready to believe in spasm of coronaries as a part of a more general vaso-motor spasm, alluded to by Duroziez,* I have not yet met with any experimental confirmation of its occurrence. To accept the view that angina is necessarily associated with coronary or other organic disease of the heart or vessels is to exclude all cases of pure vaso-motory angina: we must also forget that there are a fair number of cases of fatal angina recorded in which no coronary disease or occlusion has been found, nor must we attach importance to the fact that the coronary vessels are not infrequently found to be much diseased in cases where there has been no history of anginal seizures. It is true that in fatal cases diseased or obstructed coronary vessels constitute numerically the most frequent lesion found (in fifty-one cases out of ninety-one autopsies recorded†), fatty degeneration of the heart's texture the next most frequent, after which come aortic dilatation, regurgitant aortic disease, aneurysm, &c. In fact, if the heart be sound, anginal seizures, however severe, are rarely fatal; if the heart be unsound, whatever the lesion, but especially if there be evidence of fatty degeneration or aortic dilatation, a sudden termination may, in any attack, however slight, be feared.

Dr. Walshe, who maintains that in every case of angina there is some organic change in the heart, aorta, or both, is nevertheless strongly of opinion that there is a neurotic disturbance beyond this change as an essential element in angina. There are, he observes (p. 200), "few, if any, structural diseases either of the heart, its orifices, and its nutrient arteries, or of the aorta, which are not to be found recorded in the narratives of the post-mortem examinations of different victims of angina pectoris. Secondly, there is no conceivable disease of those structures and parts which has not in various individuals reached the highest point of development without anginal paroxysms, even of a slight kind, having occurred

* 'Union Médicale,' p. 590, Paris, 1887.

† These cases consist of those tabulated by Forbes, and the fatal cases since recorded.

during life; to this proposition extensive calcification of the coronary arteries perhaps furnishes a solitary exception."

Gairdner* with Walshe claims for angina a place among the neuroses, from (1) the fact of the pains suddenly coming and going, (2) the paroxysmal character of the symptoms other than pain, (3) the absolutely sudden death in a few cases.

Let me now relate a case which I cannot doubt to belong to the present category, yet which presents some clinical features connecting it with the first group.

Mr. C——, aged 64, consulted me on account of attacks of breathlessness, especially during the cold weather. He was a bright-featured man of sanguine and nervous temperament, spare build, and much energy of character. His working life had been spent in New Zealand, where he had—with much rough toil in earlier life—developed a large sheep-farming estate, from which he had retired, and come home some few years ago apparently in good health.

Three or four years ago, whilst walking up a hill in the late afternoon, after taking a cup of tea and buttered toast, he felt severely painful oppression over the upper sternal region, with difficult breathing and pain down both arms. He was obliged to stop until in five or ten minutes the pain passed off, and he continued his walk up the hill and beyond for another two miles without further trouble or exhaustion. He suffered several such attacks about that period, mostly coming on after tea, and attributed by him to indigestion and exercise. He could mention, however, no symptoms of disordered digestion except slight flatulent eructation at these times. The next date he could fix upon was last year during "a twenty-four mile walk in Italy with his daughter." At the end of twelve miles he came to a hill and experienced the same symptoms of oppressed breathing, pain of dull severe character over the upper sternum and down the arms, causing him to stop a minute or so. He resumed his walk, but was stopped three or four times in the quarter of a mile up that hill by the same symptoms. He then continued his walk down the hill beyond and on the level for another "ten miles." He felt fairly well after this date until the oncoming of the recent severe weather in November. One day, whilst walking after luncheon in the country, he felt the "cold wind pierce through him," causing a recurrence of the symptoms above described. He had to stop three or four times, and then walked on three or four miles without further difficulty. He has suffered several such attacks recently, and his wife informed me that they had come on whilst walking quietly, and on the level, and that during the earlier attacks she had observed him to look very grey and latterly somewhat purple in the face, and that he looked very ill all the rest of the day.

Now the family history of this gentleman reveals that his father dropped down dead whilst out walking, his mother died of some chronic heart disease, and his brother died at about 50 after repeated heart attacks. On examination this gentleman had a slow regular pulse, 59 to the minute, at first soft, *but when felt later in the interview, after a walk up and down*

* Reynolds's 'System of Medicine,' vol. iv, p. 570.

the room, of notably increased tension, but still compressible and without reflux. Hands observed to be somewhat dusky in colour. The heart's apex-beat was not very distinct, but was made out by percussion dulness and palpation to be in the nipple line. The upper margin of complete dulness was at the fourth rib, and the area of dulness was thus considerably increased.

This case and that first related, while they resemble each other in some particulars, differ also in some important respects. The age and sex of the two patients differ. While exercise in both cases will bring on the attack, in the one the symptoms will pass away while the exercise is continued, in the other the patient is obliged to stop—although in the earlier periods of his malady he could, when the attack had subsided, continue his walk.

In the one case there is evidence of an enlarged weakly and slowly acting heart, in the other of a labouring quick action with a high degree of resistance. Looking critically at the two cases, however, the difference between them is, as regards mechanism, only one of degree. In the one case the acute distress arising from cardiac strain due to sudden tightening of vessels always more or less at high tension, the vaso-motor nervous system being always more or less hyperæsthetic, the heart over-taxed but not damaged as regards nutrition. In the second case the attacks being started by exercise after food, by chill, &c., and the variable degree of arterial tension noticed during examination, point to essentially the same mechanism, namely increased vascular tension and suddenly increased resistance to circulation; but the difference and the urgent danger of the attacks in the latter case consist in this, that the sudden call is made upon a feeble degenerated heart with in all probability the circulation through it permanently impaired, possibly through organic narrowing of the coronary vessels.

In the first case the prognosis is favourable for many years, with a probability of death ensuing from some other cause, such as cerebral hæmorrhage. In the last case the prognosis is almost certainly fatal within a short period.

There is hereditary predisposition in both cases, but very strong in the second. Huchard well points out that the disease angina is not in itself hereditary, it is the diathesis which tends to produce aortic or arterial lesions, and we may add excessive arterial tension, which is hereditary.* Huchard explains the relative frequency of angina amongst professional men—financiers, politicians, doctors,

* Huchard, *loc. cit.*, p. 397.

&c.—as due to increased arterial tension, predisposing to arterial sclerosis and angina pectoris. If we further admit the pure vaso-motory variety of angina, the prevalence from the above-explained cause would be still more marked.

It is all-important then as an element in prognosis, and I may add in regard to treatment also, to make out as precisely as possible the condition of the heart. In some cases aortic regurgitation with dilatation of the vessel is present; these cases, to which I alluded last year as cases of spurious aneurysm, are obvious enough. They frequently terminate with angina, and in their course angina is frequently met with and proves amenable to treatment. Indeed it is interesting to observe that the anginal attacks in this class of heart disease are, characteristically, disorders of the vaso-motor mechanism. It was in a case of this kind that Dr. Lauder Brunton made the well-known observation, based on physiological experiment, of relaxing arterial spasm and giving instant relief to the symptoms by the use of nitrite of amyl inhalations, thus inaugurating for our clinical use that valuable drug and its allies. When one observes the violent exercise to which the arterioles are subjected in aortic regurgitant disease, one cannot wonder at arterial spasm being sometimes started, nor can we be surprised at the sometimes fatal effect of such suddenly increased blood pressure upon the already over-taxed ventricle.

The cardiac signs in angina are commonly very difficult to estimate, however. The first point is carefully to make out by palpation, percussion, and auscultation the exact point of the apex-beat, remembering that in these cases, from the age of the patient and other causes, a certain degree of emphysema of the lung is frequently present, obscuring the heart's beat. In the cardiac cases we are now discussing, the apex-beat is generally low and to the left of the normal. The area of cardiac dulness is frequently increased upwards by a rib's space. The cardiac impulse may be diffused, it is rarely strong, and an obscure and weak impulse with increased size (allowing for possible emphysema) means a large, feeble, probably degenerated heart. The most common murmur is a soft mitral,* and a soft apex systolic murmur in the absence of any history of rheumatic fever is an almost sure

* In this point, and in regard to the relative importance of aortic stenosis and insufficiency of aortic regurgitation, my experience differs from that of Dr. Walshe.

sign of degenerative changes. There may be no murmur, however; sometimes the first apex-sound is soft and very indistinct, suggestive of increased size with diminished function. More often, however, the first sound, instead of being a combination of a muscular and a valvular sound, is short, sharp, feeble, but purely valvular, not to be distinguished except by time from the somewhat accentuated second sound which closely follows it. There may or may not be a rough systolic bruit at the base. I have not observed this murmur to be a frequent or important sign in angina, whilst it is very frequently present in elderly people whose hearts give them no trouble. The irregular and, as a rule, slow pulse of cardiac degeneration contrasts with the rapid, sometimes tumultuously rapid, pulse of the vaso-motor variety. The pulse is often of variable tension.

I cannot pause to say much with regard to treatment of the two varieties of angina just reviewed. I may briefly say, however, that nitrite of amyl and its still more reliable ally nitro-glycerine are of great value in the majority of both sets of cases; but far more so in the graver cardiac cases than in the purer vaso-motory. In the vaso-motory group they require to be combined with nervine tonics or sedatives, iron or zinc with valerian, bromides, &c., and often with measures of a depletory kind: stimulants are best omitted and exercise encouraged. In the cardiac group carminative stimulants are of special value, and with them nitro-glycerine may be combined. Digitalis guarded by nitro-glycerine is often of great value, and of all tonics arsenic is as a rule the best. Only the gentlest exercise must be permitted, never straining or forcing the pace; all the causes of nervous or vascular excitement must be removed as far as possible. In one case large doses of nitro-glycerine gradually increased from one to seven minims of 1 per cent. solution were given thrice daily by Dr. Morton, of Croydon, with whom I saw the case in consultation, and during severe attacks Dr. Morton even gave twenty-five- and thirty-minim doses, and repeated them in an hour, with excellent results. This gentleman finally died of syncope. I have not yet seen another case calling for such large doses, which would of course *only be given with great caution by gradual progress from smaller doses.*

The following case appeared to me to be one of the less common forms of *cardiac angina*, in the mechanism of which vaso-motor spasm was not so distinct a factor as in the preceding:—

A gentleman, aged 55, was brought to my house in a cab by his wife, and, as I was engaged in my morning's work, my servant, alarmed at his apparent illness, showed him into my third room, where I saw him as soon as possible.

I found him sitting on a chair leaning forward with his hand pressed to his breast, with pale pinched features beaded with perspiration, while the surface and extremities were cold, the pulse was small, thready, and irregular ; a half-suppressed groan, and the expression of his countenance, bespoke the severity of his suffering. After a restorative and a tabloid of nitro-glycerine he was gently moved to the sofa of my consulting room ; some more nitro-glycerine and a little subcutaneous morphine were given, and he was able to lie down. The pulse became more regular and less thready—it never had the perfect character of vaso-motor spasm—but the pain continued severe with intervals only of diminution. I gave a small subcutaneous injection of morphine, and continuing my engagement in another room I looked in upon him every few minutes, and on doing so thirty-five minutes from the commencement of my observation, I found that he had just vomited slightly and expressed himself as for the first time feeling decidedly easier. His pulse had become regular and of better force, but whilst I was in the act of re-assuring him on these points he uttered an exclamation, "Oh-h-h !" clasped both hands to his breast, partially raised himself on the couch, and then fell over on his left side, livid, insensible, pulseless, and with breathing arrested in expiration. He was from that moment dead, and although by slapping with cold water some deep convulsive respiratory movements were excited, and some loud groans escaped with expiration, the pulse never returned. Death at the heart was already present ; the jugular veins became slowly distended and could not be emptied by pressure towards the heart.

This gentleman was a military man quite recently home from many years' service in India, during which he had suffered malarial troubles. He had dined with family friends the evening before in apparent health and good spirits, discussing the question of retirement or continued service in India, and had never previously complained of any heart symptoms or been known to have any cardiac weakness.

At the moment of attack he was walking quietly with his wife from his hotel towards the station to meet his children, whom he was intending to take across to another station, *en route* for a long journey to a country house which he had taken for them. This must have been about an hour or two after breakfast, and he had complained of nothing until the time of seizure.

I cannot formulate to myself the mechanism of this appallingly sudden death, the painful surroundings of which prevented me from urgently suggesting a post-mortem examination.

The notable manner in which the jugular became filled and remained so, resisting pressure onwards, suggested to me that the

right ventricle became first paralysed. The radial pulse ceased almost instantaneously with the last agonising pang.

Latham accepts the definition of spasm as being "a mode of action in muscular structures different from or beyond the natural and accustomed mode," a condition always accompanied by pain and disablement. The heart in structure and function is a muscle. "In its spasm of smaller degree the heart fails to close freely upon the blood and to impel it freely into the arteries. In its spasm of greater degree it fails to project it altogether. Herein we discern an adequate explanation of the chief phenomena of angina pectoris. It is a spasm of the heart."* Dr. Latham's remark seems to me to apply very accurately to this case, in which the spasm and failure commenced at the heart, in which the small thready pulse was due to the diminished blood-volume sent to the vessels by the disabled heart, which failed absolutely with a spasm of greater degree. There can, I think, be little doubt that this gentleman had diseased coronary vessels, and that some sudden accession in the shape of thrombotic closure or blocking by detached atheromatous *débris* occurred, the pain being in part analogous to that severe anguish with which we are familiar in the limbs as premonitory to gangrene arising from arrested circulation in them. Nitro-glycerine had no distinct effect in this case.

Unquestionably direct or reflex irritation of the cardiac plexuses may produce angina. No doubt some of the cases of angina in association with aneurysm and more acute cardiac irritations are of this order. I saw, three years ago, with Dr. Elliott, of Southampton, an interesting example presumably of this kind.

A lady of nervous temperament, approaching the menopause, had had three or four severe attacks of angina in quick succession. She was the subject of a chronic tuberculous affection of the left lung, upon which had supervened an acute and no doubt tuberculous pericarditis, which latter affection accounted for the anginal attacks. All her symptoms subsided, and she has since, although still in delicate health, been free from angina.

I occasionally see a lady, aged about 55, who has for many years suffered from left renal colic. Her right kidney is decidedly enlarged, but of normal shape, smoothness, and consistency, although considerably movable. The left loin is comparatively empty and no kidney can there be felt. It is my belief that she has a shrivelled calculous kidney, which has not lately caused her suffering, but she now frequently suffers from a mild degree of vaso-motory angina. It is a question how far this may be the result of reflex irritation.

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* Latham, 'Diseases of the Heart,' p. 461.

I shall only trouble you with yet one further variety of angina, viz., *syncopal angina.* The variety corresponds with the *angina sine dolore* * of Gairdner, and in its essence consists of a prolonged intermission of the heart's action.

This form of angina is of rare occurrence, except in persons over 65. It is usually associated with gout. The subjects of it have slow, feeble, and often irregular pulses, with sometimes a peristaltic contraction of ventricle between the beats.

It has been ascertained by experiment † that unduly acid blood —whether from excess of carbonic acid or from the presence of lactic acid—bathing the ventricle will cause the heart to stop. It is known the sub-endocardial tissues of the heart are nourished by permeation of blood into a spongy network of fine channels interwoven with the sub-endocardial muscular fibres. We have in these two facts a possible explanation of the syncopal attacks which sometimes occur in the subjects of gout and in persons cyanosed in acute pulmonary disease. Acute dyspepsia from overloaded stomach, acute intestinal catarrh, and nervous shock, are the most common exciting causes of the attack, however, and it is probably a question of age, temperament, and relative sensibility of the two nerve-systems regulating the heart and vessels whether these people should present to us the more stormy phenomena of anginal spasm or the quieter and painless distress of syncopal failure. Careful feeding without long intervals and in small meals, two hours' reclining daily before meal times, with an occasional twenty-four hours in bed, the gentlest level exercise, peptic tonics, and during attacks hot drinks and carminative stimulants, are the points to have regard to in treatment.

I have thus endeavoured to argue that angina pectoris is a disturbed innervation of the heart or vessels, associated with more or less intense cardiac distress and pain, and a general prostration of the forces, always producing anxiety, and often amounting to a sense of impending death.

(1.) In its purer forms we observe disturbed innervation of the systemic or pulmonary vessels causing their spasmodic contraction, and consequently a sudden extra demand upon the propelling

* The term *syncope anginosa* adopted by Parry from Cullen (1799) would be applicable rather to the preceding variety of *angina pectoris gravior.*

† Gaskell, ' Journal of Physiology,' vol. iii.

power of the heart, violent palpitation or more or less cramp and paralysis ensuing according to the reserve power and integrity of that organ—*angina pectoris vaso-motoria.*

(2.) In other cases we have essentially the same mechanism but with the extra demand made upon a diseased heart—*angina pectoris gravior.*

(3.) The trouble may commence at the heart through irritation or excitation of the cardiac nerves, or from sudden accession of anæmia of cardiac muscle from coronary disease—*primary cardiac angina.*

(4.) In certain conditions of blood or under certain reflex excitations of the inhibitory nerves, always, however, with a degenerate feeble heart in the background, we may observe intermittence in its action prolonged to syncope—*syncopal angina.**

Dr. Ord insisted on the remarkable parallel between asthma and angina, and he pointed out that the term asthma formerly comprehended a great number of varieties of difficulty of breathing. It had been pointed out by statisticians that in times past asthma was a very fatal disease according to the returns; its having become less so was probably due to better diagnosis. With regard, however, to angina, the condition was rather different; at present, people would have a larger range for angina than formerly, especially if they adopted Dr. Powell's conclusion. The old limited definition to which he alluded would certainly have to be extended to a larger number of cases in which one or two only of the constituent typical symptoms of the disease as first described would be present. Such cases might perhaps be termed "anginous" or "anginoid." He then entered upon the consideration of the immediate causes of this condition, comprising vaso-motor disturbances, arterial tension, heart weakness, either of wall or of valve, or of both, and, lastly, those due to inhibitory and depressing influences. He pointed out that these anginoid attacks usually came on in the day-time, and the prognosis was notably aggravated when they occurred at night. He compared the two classes of cases, one in which the patient had angina on exertion as the consequence of sudden arterial tension; these patients very commonly had a weak heart, and so the extra strain thrown on the right side of that organ by the exercise precipitated the anginal attack. The other group was that of patients who had the attack independently of muscular exertion, and in these he thought the mechanism was probably through the nervous system, either emotionally or by reflex. He had seen illustrations of both classes. One of the aggravated conditions of attacks primarily anginous and determined by movement was the occurrence of these attacks by night. He dwelt on the importance of dyspepsia and constipation as factors in bringing about the attacks, and observed that if he were restricted to one remedy in the treatment of angina he would prefer sulphate of magnesia to nitro-glycerine. He mentioned the

* This group would include the *vagus angina pectoris* of Ross.

frequent coincidence of glycosuria with angina, which, he thought, authorised the inference that the visceral blood vessels were dilated and not contracted, as the author seemed to suppose, along with the superficial vessels. He said that the connection of gout with angina ought not to be forgotten, involving as it did a neurosis and changes in the chemical constitution of the tissues. Consequently treatment directed to the gout often relieved the angina. He insisted upon the fact that no sensations of anginous type were to be treated lightly, or the patient left without a proper explanation of the danger, with the precautions which became necessary.

Dr. Broadbent said he had been unable to make up his mind as to the condition of the heart during the paroxysms. Sometimes the pulse was irregular and small, sometimes there was high arterial tension, sometimes not, and sometimes the pulse was practically unaffected throughout an attack even of great violence. The central fact in attacks of angina was that the heart was beaten, either by peripheral resistance or on account of failure of its own powers, and that fact fitted in very well with the author's classification. It was especially in those cases due to weakness of the heart that great danger arose. He pointed out that vaso-motor resistance alone would not be sufficient to determine the attacks. The proportion of cases of high arterial tension without angina pectoris was very great even when dilatation of the heart supervened. A second factor was required, probably degeneration. He presumed that disease of the coronary arteries acted by causing degeneration of the muscular fibres. As explaining nocturnal angina when it was not due to the effect of undressing or the contact of cold sheets in bed and during sleep, there was the upward pressure of the abdominal viscera and the curious running down of the cardiac powers to account for the supervention of the attacks. Some cases commenced with acute aortitis, leading to interference with the coronary circulation, causing a series of anginoid attacks, resulting in physical evidence of progressive cardiac failure. Angina, he believed, had sometimes its origin in failure of the right ventricle, and he pointed out that muscular exercise lowered very rapidly the resistance of the peripheral circulation, and the effects of exercise were mostly felt on the right side of the heart. With reference to the neurotic theory, he pointed out that the vast majority of anginal cases did not occur in the neurotic sex, for it was extremely rare in women. He bade them, therefore, beware of attaching too much importance to an unexplained neurosis. The occurrence of angina in gouty people he ascribed to its association with high arterial tension and secondary degeneration. In the diabetes of advanced life there was also usually high internal tension which might contribute to the production of angina, and he had noted the existence of a myocarditis in patients suffering from glycosuria, and this was usually associated with marked anginal paroxysms. He remarked on the fact that angina was seldom or never associated with mitral lesions, and anginal symptoms sometimes subsided on the supervention of a mitral lesion in a person subject to anginal attacks. His own experience seemed to show that the gravest cases of angina were those in which no physical signs could be found to account for the symptoms.

Sir Walter Foster agreed with Dr. Powell that a broad basis should be taken for the consideration of this subject, for under this title could be grouped a series of conditions from trivial remediable disorder to severe attacks of terrible heart pang, ending in sudden death. If they were con-

sidered as a whole, some ideas might be built up as to their pathology and treatment. He first desired to refer briefly to four cases. The first was that of a doctor, aged 64, who complained of angina pectoris, which came on in the course of the night. The symptoms were described as those of intense pain, which developed in the region of the nipples and radiated down the left arm, accompanied with terror and a sense of impending dissolution. Under sinapisms and free stimulation he was relieved. It was in this patient the earliest symptom of a gouty condition, whilst later the attacks were induced by walking up hill. The patient died of a dilated heart eight years afterwards. He recalled another case which occurred in the year 1863, when he was in charge of a friend's practice in the south of London. It was that of a young man who was a good runner, and who one evening ran the length of the street for a wager. When near the end of the street he uttered a sudden cry, and died in a few minutes. A careful necropsy revealed the left cavities empty, the right ventricle almost so, while the right auricle was enormously distended. In his death agony he complained of intense cutting pain in the chest. In another case a prematurely old man, aged about 56, complained of intense pain across the chest; the pulse was slow, not more than 50 to the minute, and was of high tension. During the attacks the pulse fell to 30, and became intermittent; there was pain between the nipples, down one or both arms, together with much anxiety, general clammy sweats, and pallor of the face. Against advice, he married a young wife, and though he survived very well the earlier weeks of matrimony, he died eleven months later in an attack brought on by exertion, and at the necropsy no valve lesion was found, and there was no aortic atheroma, but a dilated and degenerated left ventricle. In another case, which occurred in a medical man, he himself graphically described the attack. The pulse fell to 20 at the wrist, though the heart was beating more frequently, and it was small, irregular, and hard; the face was pale, there was no pain in the chest, but he felt that he was dying. Sinapisms were applied to the chest, and hot bottles to the feet, while strophanthus, with ether and ammonia, were given internally. Under this treatment he recovered; he put it down to a neurotic attack induced by influenza, and influencing the pneumogastric nerve. Later he had a second attack, slighter in degree, but he died in the midst of a third, which came on at night, and was associated with well-marked heart pang. No necropsy was made, but there was no indication during life of organic disease of the heart or arteries. These cases illustrated certain features of the affection, and he wanted to compare them with other cases—cases in which there was agonising pain accompanied by grave organic disease. In one man, who committed suicide on account of the anguish, there was double aortic valve mischief, with an aneurysm springing from one of the sinuses of Valsalva, and pressing on the root of the aorta and on the cardiac nerves. In the cases of valve disease which suffered the most pain there was nearly always dilatation of the left ventricle of the heart, with, later, degeneration of the cardiac muscle. This dilatation was usually associated with aortic disease, and confirmed Dr. Walshe's remark, that the cases of disease of the basal valves were those associated with angina. He had once witnessed a case of rapidly developing insufficiency of the aortic valves produced by a sudden strain. The patient had severe chest pang, and was collapsed and insensible. He was bled from the arm and rallied, and was then found to have aortic regurgitation, although he was known before the strain to have had sound valves. For days he complained of

stretching pain under the sternum, due to dilatation of the ventricle. The same kind of stretching pain was met with when further dilatation was in progress in an already diseased heart, as when the hypertrophied muscle underwent degeneration late in Bright's disease ; and such cases were relieved by the establishment of mitral regurgitation. In all these cases he had related he believed the immediate cause of the heart pang to be distension of the cavities of the heart, either right or left. In the case of the patient who got his attacks when walking up hill, the right was involved, while in the gouty patient, in whom the attack came on at night, it was the left ventricle which could not overcome an increased vascular pressure, brought about, perhaps, by the circulation in the blood of some excess of uric acid. He held that stretching, and not spasm, was meant by the "distensio" of Heberden. He thought that in all, or nearly all, the cases there was a degeneration of the myocardium, with distension of the cardiac cavities. The order of events was (1) degenerating myocardium, (2) vaso-motor contraction, (3) over-distension of the weakened heart cavity. The weaker the myocardium, the more dangerous, *cæteris paribus*, the distension. This view was counter to that of neurosis, but till it was demonstrated he could only regard the idea of spasm of the coronary arteries as a fascinating hypothesis. In Sir Richard Quain's revised article on angina, he referred to Dr. McWilliams's recent researches as to the nature of "delirium cordis or fibrillar contraction" in animals in which there was a quick irregular cardiac action, with fall of blood pressure due to a distended ventricle remaining in a state of diastole and excited by trivial causes, such as distension of the gut. This condition presented, possibly, a close analogy to angina pectoris, but even this state is regarded by the experimenter as due to some malnutrition of a degenerative character. He preferred to regard angina, not as a disease, but as a mode of death and a term under which might be grouped a number of allied conditions having a common pathological basis. In the treatment a scientific step had been made when drugs like nitrite of amyl and nitroglycerine were introduced, which counteracted the vascular condition which was the exciting cause of the malady. If the pain were prolonged, morphia subcutaneously was indicated, while in the intervals no drug was more valuable than arsenic, with ether, iron, bromides, or alone. Iodide of potassium, in doses of 5, 10, or 15 grains three times a day, had wonderful power of lessening the pain and improving the patient's condition. The female sex were less troubled with angina, because they were less exposed to strain of the heart than men.

Professor Grainger Stewart found that his line of thought diverged considerably from that of the previous speaker, and he referred to the interesting comparisons that had been made between glycosuria and asthma and the affection under discussion. Cases of angina grouped themselves into four categories : 1. Simple neuralgia of the heart, seen in women at the climacteric, and in men of gouty constitution, in which pain was referred to the heart itself, and was very amenable to treatment. 2. A condition of angina dependent upon and associated with acute or subacute inflammation of the aorta. He had more than once seen a patient describe a curve on his chest like that of the aorta to indicate the site of his pain, and he described a case in which pain was referred to the lower part of the thoracic aorta which recurred in paroxysms and travelled gradually higher up. Later, a marked double aortic murmur developed, and death occurred. There was no post-mortem, but aneurysm, pleurisy, and new growth had been excluded, and he considered that the case had been one

in which an aortitis had commenced low down and had spread upwards, ultimately damaging the valves and producing dilatation of the heart. Though this latter had only developed at the last, pain had been present from the first, and had been of an anginous character. 3. A common group of cases in which angina pectoris was associated with fatty, fibrous, or other degeneration of the cardiac walls, or with changes in the coronary arteries. 4. Angina pectoris associated with spasm of the arteries and increased tension. There was very little change in the heart, but it was overmastered by the arterial spasm without the coronaries or substance of the heart being necessarily involved. As to the determining causes of anginal attacks, many of them would apply to all four clinical groups. They were exertion, emotional excitement, such as anger or grief, dyspepsia, constipation, the various toxines absorbed into the system from the intestines, or remaining in the blood, from failure of the kidneys or other eliminants, and certain changes in the nervous system, such as, for instance, that which occurred when sleep was setting in; any one of these might suffice to bring on an attack. But in any of the last three pathological groups the same lesions might exist without pain; so that one had to ask one's self, What was the explanation of the pain when it occurred? Was it of muscular or of nervous origin? Sir Walter Foster had said that it was a muscular spasm, but he did not know what spasm of the heart muscle meant; he doubted very much if it were like cramp, though a vague idea to that effect still floated in the minds of some. What seemed against this was that the heart and pulse might remain steady all through an attack, which could not be the case if a cramp-like condition existed in the cardiac muscle. And certainly if, as he believed, aortitis could of itself be a cause of angina, muscular spasm could scarcely be held to account for it, for that vessel did not contain enough muscle to produce a painful cramp. He therefore looked upon it as a pain of nervous rather than of muscular origin. Inquiring whether any purely nervous diseases resembled angina, he pointed out that epileptiform neuralgia was wonderfully like it, coming on in sudden paroxysms, and often ceasing as suddenly, leaving the patient well, but subject to a new attack, being more common in men, and at or after the middle period of life, while certain slight peripheral influences were capable of inducing intense pain. He discussed in some detail the pathology of epileptiform neuralgia, and argued that, although it was no doubt central, it was largely influenced by peripheral conditions, as was shown by many facts as to its clinical history and treatment. He argued that an analogy existed between this and angina pectoris, in which explosions of pain occurred from time to time in connection with irritation in the aorta or heart. In short, he suggested the essence of the process to be a change of organic or functional nature in the nerves or nerve-endings which controlled the heart, or in the centres connected with them. For treatment, he agreed that the general health should be improved, excitement and effort being avoided, and nitrite of amyl administered to relieve during the paroxysms. The latter drug he believed had also direct effect on nervous structures, for it relieved other neuralgiæ. Arsenic and iodide of potassium were invaluable, and occasionally direct cardiac tonics, such as digitalis and strophanthus, might be given with benefit.

Professor MacVail agreed that the basis of the whole subject should be broadened, so that any condition of pain, except that of inflammation of the pericardium or endocardium, about the heart should be alluded to as angina. As regarded the pain, it might be general or partial. He

had two cases in which it was likened to a knife plunged into the apex of the heart; in another it was as if needles had been driven in; in another as if crushed or grasped; and in yet another as if greatly dilated, the patient actually feeling to ascertain if his chest was not bulged out. In some the pain was radiating, in others it was distinctly local. The question of radiation depended on two circumstances: on the amount of the pain and on the ability of the nerves to conduct. People differed from one another, and the same people differed at different times with regard to their power of conductivity. In one of the cases above mentioned, in which the pain was knife like, there was no radiation; but in the second case of knife-like pain it passed from the cardiac apex to the left shoulder. In the case in which the pain was like needles it passed from the apex of the heart to the scapular angle, thence to the top of the left shoulder and down to the bend of the arm. In the case in which the heart felt to be dilating, the patient either became faint or was confused mentally; this might be due either to an insufficient blood supply to the brain, or it might be really an instance of radiation, and thus resemble some of the mild forms of epilepsy. Not only did the pain radiate from the cardiac seat, but before the attack began a sensation might also travel towards the cardiac area. It was a kind of aura. In one case, that with the crushing sensation, the patient described a sensation as of burning or of boiling water poured on a small area of skin near the umbilicus; thence it travelled towards the heart, and then the attack of angina commenced. In the case where the pain was like needles it commenced at the inner end of the left seventh interspace; thence it travelled to the apex of the heart, which took five minutes; from there the pain went to an inch below the scapular angle, taking ten minutes, and it was now no longer like needles, but like a knife; thence to the left shoulder in five minutes, and finally to the bend of the arm in two minutes. The connection between angina and epilepsy was worth more attention than had been paid to it, and in every case an antecedent sensory condition should be inquired for. He had found the organic conditions present to vary much; in one of the cases of knife-like pain there was a double mitral murmur, while in the other there was mitral regurgitation alone. In the first instance, where the sensation was of a crushing character, there was a double mitral murmur; in that in which a sense of dilatation was complained of the condition was one of double aortic murmur. In the case where the pain was like needles no organic abnormality could be made out. What was the actual condition of the heart when the pain was present? Sir Walter Foster and Professor Stewart had contended that it was not cramp. Was it possible to separate cramp from a neurotic condition? Though it might be done in laboratory work, it was quite impossible to separate them in practice. He himself was quite unable to separate the nerve condition from the muscular condition of the heart as regarded pain. An ordinary muscular contraction was a pleasurable sensation, as in exercise; but if the contraction were much increased in intensity, as under strong faradaism, it then became painful and was a condition of cramp. The muscular condition in the living body was always one of *tonus;* there was no such thing as complete relaxation during life. It had long been taught that the cardiac contraction was not one of physiological tetanus, but this idea had been proved beyond all dispute to be false. If over-contraction were present, it was simply a question of degree whether pain would be present or not. Dr. Walshe had objected to angina being due to spasm, because he held that if it

were due to spasm the heart would necessarily remain continuously contracted during the spasm. But this by no means followed, for, if the upper extremities were thrown into a condition of tetanus by means of the faradaic current, it would be still found possible to move the hands, unless, indeed, the current were extremely strong ; so in moderate cases of angina the tetanic spasm was not strong enough to interfere with the cardiac rhythm. Dr. Walshe likewise stated that as in diastole the ventricle was flaccid, the angina, if due to spasm, should be intermittent. But there was no evidence to show that the ventricular wall ever became flaccid even in diastole. The presence of a murmur in aortic regurgitation was fatal to the idea of the ventricle ever giving up its grip of the blood. The idea of this murmur being caused by valve vibration was of course absurd, and Balfour had shown that it was caused by a thin stream of regurgitant blood forcing its way back into a mass of blood remaining in the ventricle under considerable tension. If the ventricle were thus never quite flaccid even in diastole, it removed the great objection to the theory of spasm. He thus upheld the old idea of Heberden, as supported by Latham, that the condition of angina was immediately dependent upon spasm of the heart. The excessive irritability of nerve and muscle caused by overwork induced instability of the nerve centres, and if the explosions from the unstable centres became sufficiently frequent, then a condition of cramp would result. He asked, in conclusion, that each individual case should be examined in reference to the order in which the parts were attacked by pain, and the directions into which it radiated.

Dr. Lauder Brunton observed that it was very rare for a third night to be devoted to discussion in any society, and the fact that the Medical Society had done so on this occasion showed its indebtedness to Dr. Douglas Powell for the admirable paper by which he had introduced the subject He agreed with Dr. Powell and with the other speakers that the subject of angina pectoris should be treated on a wide basis. In discussing the subject much help might be obtained from the analogy pointed out by Professor Grainger Stewart between the heart and the bladder. Both were hollow, muscular organs, contracting and dilating at fairly regular intervals and expelling liquid contents. In both, slight distension formed a stimulus to contraction, but if the distension became excessive it might cause pain varying in amount from slight discomfort to extreme agony. The sensation of pain is always central, for it depends on a condition of the brain which may be excited by a peripheral irritation like a diseased joint, but may exist quite apart from any peripheral disease, as in hysteria. A mixed condition may exist which is both central and peripheral, as in the vesical crisis of locomotor ataxia, and possibly a similar condition might occur in the heart. Usually distension either of the bladder or the heart not only caused contraction in them, but, through the nervous system, lessened the resistance of the sphincter in the case of the bladder or of the arterioles in the case of the heart. Disturbance of this nervous relationship might cause excessive distension or pain in either organ apart from organic disease, giving rise to a spasmodic retention or vaso-motor angina. But while the spasmodic character of angina pectoris indicates its nervous origin, it is so frequently associated with diseased coronary arteries and fatty heart that these evidently play a great part in its causation. Angina pectoris may be said to be due neither to high tension alone nor weak heart alone, but to weakness of the heart in relation to the resistance it has to overcome, and it may be brought on either by weakening the heart or increasing the resistance, or

both together. In tracing out its pathology the analogy with the bladder is useful. The resistance which the heart has to overcome depends upon the contraction of the systemic arterioles. Of these there are three subdivisions :—Cutaneous, visceral, and muscular, and the latter is the most important in relation to the attacks of angina. One of the most striking symptoms of angina is the ease with which it is brought on by muscular action. The first effect of this is to prevent the passage of blood through the muscles and to raise the tension, although, afterwards, the vessels of the muscles dilate and the tension falls. To this primary rise Dr. Brunton attributed the occurrence of anginal attacks on exertion, and their passage off if exertion could be continued was, he considered, due to the subsequent dilatation of the vessels of the muscles, with consequent fall of blood pressure and lessened resistance. He thought that atheroma of the coronary arteries was so powerful in causing angina because it prevented the supply of blood to the heart being increased on exertion as it ought to be in muscles generally. The pain of angina he considered to be due to distension of the heart, and its more common occurrence in the left than the right side of the heart to be due to the greater ease with which the tricuspid valves became incompetent than the mitral when the ventricles became dilated. Angina pectoris is very rare in children, but one case is recorded of a boy of 14, in which there was aortic regurgitation and mitral stenosis, and another in a boy of 11, where there was ossification of the auriculo-ventricular groove, conditions which would prevent dilatation of the mitral orifice and insufficiency of the valves. He agreed with the previous speakers in regard to the necessity for the elimination of waste products, and also for restricted diet in gout and for the use of iron and arsenic in feeble heart, but he regarded the nitrites as the most efficient remedy during a paroxysm and, on the whole, iodide of potassium as the most useful in the interval. He showed a tracing which appeared to indicate that it lessened the rise of blood pressure on exertion, and mentioned a case illustrative of its remedial action.

Dr. Mitchell Bruce said that he intended to confine his remarks to some practical clinical points. He dealt first with so-called "vaso-motor" angina pectoris. He had seen many cases of this character, but the majority were associated with cardiac lesion ; in few, if any, the heart was sound. He did not gather from Dr. Powell what proportion he considered these two groups bore to one another, but in his own experience cases of simple "vaso-motor" angina were excessively rare. The term "vaso-motor angina pectoris" he ventured to consider an unfortunate one, as it was apt to be misleading. An attack of angina was determined by the relation of the arterial tension in front to the force of the heart behind it. It was really a matter of relation between the pressure ahead and the driving power ; and, whilst acknowledging that a sudden rise of pressure from any cause determined the attack, he maintained that in the great majority of instances the fault lay with the heart, not with the pressure. If the term "vaso-motor angina pectoris" were used to express this condition, it overlooked the most important factor in regard to both the pathology and the treatment. He had not found the pulse particularly tense in patients with vaso-motor angina ; they possessed usually rather a soft and poor pulse. In aortic cases with angina the pulse was, as a rule, anything but tense ; and in Bright's disease the angina only came on when the heart began to fail. If the heart were carefully examined in most of these gouty subjects, there would be found physical signs of some organic change : not of active hypertrophy, but of enlarge-

ment with weakness. The typical people who suffered from gouty angina were men between 40 and 50 years of age, who had led an anxious but sedentary life, and who, having been successful, felt themselves entitled to ease and free living. Such persons presented symptoms of irregular gout, complaining of dyspepsia, diarrhœa, insomnia, eczema, and flying pains. They had palpitation, sometimes actual angina, and were despondent and nervous, with a feeble pulse. One point on which he desired to dwell was that in these subjects the first attack of angina was often associated with a definite act of over-exertion. In many cases they were stout men who, in order to counteract their obesity, took to exercise in an ill-judged fashion, such as excessive rowing, tricycling, &c., and in this way strained themselves, and afterwards became the subjects of gouty heart; here, as elsewhere, the gout "settling" in a strained organ. Again, it had often been noted that before an attack of acute gout there was increased pulse tension, but during this period of increased tension the patient was better of his angina. Lastly, the effect of treatment bore out the importance of keeping the cardiac element well to the front. Under certain circumstances iodide of potassium was more valuable than nitro-glycerine, and between the attacks regular gentle muscular exercise was good, the patient attending strictly to his diet. As to purges, he agreed with Dr. Ord as to their value, and if in the treatment of this affection he were limited to one drug, he would select blue pill, which acted wonderfully in irregular gout with angina, though he was unable to explain its method of action. Coming next to classical angina, he had been struck with the frequency of the small attacks and with the comparative rarity of the large and severe ones. The occasional appearance of gastro-intestinal phenomena in both gouty and organic angina should be borne in mind. He referred to the case of a man, aged 56, with "weak heart" and an intermittent mitral murmur, who suffered from attacks of angina both of slight and of violent degree. One evening, when walking home on a level road, he was suddenly seized with increased pain in the chest and left arm. He passed a large motion, became unconscious, and this was followed by vomiting. The temperature rose, cough developed, and moist sounds were present over the base of one lung. He had known of a similarly sudden development of gastric and intestinal disturbance in three other cases. Lastly, he considered that pseudo-angina pectoris was deserving of some attention in a debate on the subject.

Dr. de Havilland Hall said that, in looking over the old claims of his assurance office, he had come across several instances of death attributed to "gout of the stomach." According to Brinton, many of these so-called cases of "gout of the stomach" are due to angina or gall-stone colic. Dr. Hall laid stress upon the ease with which these two disorders may be confounded, and he mentioned a case in which the patient, who was thought to have had angina pectoris, subsequently had an attack of gall-stone colic followed by the formation of a hepatico-bronchial fistula, and on recovery from this attack the anginal symptoms ceased. Dr. Hall insisted on the importance of patients liable to angina being kept warm, as he had known instances of the attack being brought on by exposure to cold, and he said that the cases in which cold acted as an exciting cause were those in which, as a rule, nitro-glycerine acted most efficaciously.

Dr. Seymour Taylor said the subject of angina pectoris was a wide one, and one that apparently could not be confined within the four corners of an ordinary discussion. From careful study of clinical cases he had noticed,

and after perusal of much literature on the subject, it appeared to him that one must group several states or conditions under the head of angina pectoris, and that this nomenclature merely labels as it were a condition rather than a disorder, and that it is a symptom rather than a disease. It therefore follows that the subject has a wide pathology, although it may present several changes which seem to be accentuated in the heart and large vessels. If he were to attempt to classify the various causes of angina pectoris, it appeared to him that one might group them under three headings :—(1.) Those cases which are due to manifest disease of the cardio-vascular system, whether it be of heart valve, heart wall, larger vessels, or arterioles. (2.) Those cases which are associated with some obvious neurosis. (3.) Those cases which are reflex to some internal condition of stomach, intestines, or other abdominal organs. As regarded his first list of cases, the period of life at which the condition prevails is noteworthy, occurring most frequently as it does after 50 years of age, at a decade which leaves a balance on the wrong side. Still, cases have been observed in young children by Moxon and others, but even in these post-mortem examination has shown disease of the aorta (generally and inflammatory change) or secondary stenosis. An exciting cause is hyper-activity of heart's muscle on an already over-strained or unprepared organ. Thus it may be akin to cramp in ordinary muscle. The heart is not a single muscle. The view that the heart is a simple muscular bag acting only in one centripetal movement is misleading. Its structure alone teaches us differently. It consists of a numberless series of muscular sphincters, each of which has its point of attachment and its sphere of action. And in order to have a perfectly healthy and rhythmic action of the organ each muscular zone must be healthy in structure and in nerve apparatus. Otherwise, what is the result? The comparative over-strain on a part of the heart wall which is weakened or unprepared produces a spasm and pain which is distressing to the last degree, a condition comparable to cramp in an ordinary set of muscles, supervening on unwonted exertion, or to colic coming on after bowel irritation. He thought that such a state of affairs occurs in cases of sudden drowning in expert swimmers. Here a man in the enjoyment of lusty health is suddenly seized, when swimming, with an illness, which, for want of definite pathological knowledge, is called "cramp," and he sinks at once. He apprehended that it is not only the voluntary muscles which become palsied, but that the heart shares also, and the seizure, he doubted not, is attended by momentary but agonising pain. Heart pang may also be due to peripheral obstruction, existing either as rigid arteries or as tension in the arterioles. Now, the disease which most readily comes to our minds as causing arterial tension is the contracted granular form of Bright's disease. But we may overlook the fact that the circulation through the vessels of the heart itself is peripheral and that in some cases where the pulse shows no sign of vascular obstruction in the limb vessels, such a state may exist in the heart itself. This conclusion was forced on his mind by the observation of two post-mortem examinations, in both of which huge cardiac hypertrophy existed, whilst microscopical examination revealed fibrous interstitial overgrowth, with hyaline thickening of the smaller vessels of the heart itself. Yet in neither case was there evidence of Bright's disease, or of other obstruction of the circulation. One had died from cramp whilst swimming. Dr. Broadbent had drawn attention to the fact that angina rarely or never occurs in cases of mitral regurgitant disease. This, he took it, is due to the

fact that embarrassment of the left ventricle either by obstructive or regurgitant disease of the aortic valve is much relieved by the safety-valve action of the leaking mitral valve. As regards the nervous element in the causation of angina, it is undoubtedly associated with, or perhaps rather a manifestation of, various neuroses. It occurs in neurotic families, various members of which may suffer from different nerve diseases. Thus it is met with in epileptics, or it may be a purely spasmodic neuralgic condition coming on in paroxysms similar to tic, and producing irregular contraction of the heart muscle. Again it is not unknown in asthmatics; but whether it be a neurosis or whether it be secondary to an overstrained right side of the heart he could not bring himself to determine. That angina also occurs as a reflex phenomenon he was convinced. He had seen a severe case of breast pang supervene on extensive enteritis; and again one of the most alarming cases he had met with occurred in an elderly man who frequently suffered from dyspepsia, and in whom lumbago and other muscular pains were frequent occurrences. Dr. Ord had drawn attention to angina being associated with glycosuria. This he (Dr. Taylor) was sure was a correct clinical observation, and one which it would be well not to overlook. But under which category this class of case should be put, he was at loss to decide. Should we regard such cases as primary neuroses of which the angina and the glycosuria are only symptoms, or is the angina secondary to, and caused by, the increased glycogen circulating through the cardiac muscle? He would, however, draw attention to the undoubted clinical fact that glycosuria is often attended by arterial tension. On the point of treatment he had little to say. Thanks to Dr. Lauder Brunton, we have a most valuable therapeutic agent in nitrite of amyl. But it must be given in the suitable class of cases only. It is useless, and he apprehended dangerous, in cases other than those due to tension in the smaller vessels. A recent discussion at another society had drawn attention to blood-letting as a long neglected remedy, and he should venture to suggest it be more frequently tried in cases of angina pectoris due to increased tension in the blood vessels. On the other hand, when angina is due to a recognised abnormal condition of the nervous apparatus, or when it is secondary to some disturbance of the gastro-intestinal canal, it is essential that we treat such cases on the general principles which such conditions suggest. In these the nitrite of amyl will probably be useless.

Dr. Fletcher Little could not agree with Professor Grainger Stewart that angina pectoris should be classed with "epileptic neuralgia." If it was, the element of spasm against which the Professor argued could not be eliminated, because in many cases of this form of neuralgia spasm was one of the most prominent symptoms. Was there any recorded case of death from any form of neuralgia in a couple of hours? In the case of Arnold, of Rugby, death took place in this short time during the first attack of angina pectoris. He agreed with Dr. Mitchell Bruce that a violent action of the bowels sometimes accompanied the onset of angina. He had seen two such cases. The employment of remedies during the attack should be instantaneous if they are to be effectual. He trained his patients to carry the capsules of nitrite of amyl in an outside pocket, and to break them in the palm of the hand so that the inhalation should not be delayed for an instant. During the intervals he found that nitro-glycerine in lozenges in doses of 1/100th of a drop three times a-day warded off the attacks. Many of the tabloids in the market were almost insoluble, and their use only led to disappointment. Before tabloids were

employed, they should be tested by being suspended in water to prove their solubility. He had seen several cases of angina in gouty patients with a pulse of high tension, and found brisk purgation the best treatment.

Dr. Douglas Powell, in reply, said that two opinions seemed to be current as to the nature of the pain—one that it was due to spasm or distension, and the other that it was of the nature of a neuralgia. He confessed that to his own idea the theory of spasm or distension presented the most attractive features, and the more he thought over it the more difficult he found it to conceive of the pain as being of neuralgic origin. The majority of the arguments eloquently used by Professor Grainger Stewart against tension or spasm could with little difficulty be applied with equal force against the neuralgic theory. He admitted, however, the difficulty of the objection that the heart sometimes preserved its equability of action during angina, though Dr. MacVail's observations as to the condition of striped muscle under faradaic contraction to some extent disposed of this difficulty. He was unable to agree with Dr. Broadbent and others as to the rarity of mitral regurgitation in angina. He could recall instances of angina in which mitral lesion alone existed, and he thought that in the degeneration commencing in elderly gouty people the mitral valve was usually the first to give way. With regard to treatment, he gave preference to nitro-glycerine over nitrite of amyl in most instances, and it might be pushed, with great caution and strict watching, to very large doses with great benefit. In one instance under these conditions in the intervals between the attacks seven minims were given three times daily, and during the attack as much as thirty-five drops repeated at a short interval. As to the eliminative treatment, all would be agreed, and, on the question of exercise in purely vaso-motor cases, regulated exercise according to the constitution of the patient was often of great value ; but where there was definite disease of the heart or a definite rise of peripheral pressure, then the greatest caution should be observed in this respect.

Professor Gairdner, of Glasgow, who was unable to be present at the debate, wrote to say that in the article he contributed some years ago to 'Reynolds's System of Medicine' he stated his reasons for not accepting Nothnagel's angina pectoris vaso-motoria as a true clinical description of facts running parallel with Heberden's angina. He was at the present time of the same opinion, and thought the name given a most misleading one. In Heberden's angina there was, no doubt, a vaso-motor element super-induced as a rule upon organic or structural change. Perhaps the most curious point about angina pectoris which had become known to him since writing his article was in reference to the claim so generally maintained in France in almost all dictionaries and monographs, on behalf of a certain M. Rougnon, to have anticipated Heberden in the description of the disease as a cause of sudden death. He had dealt with this claim in a note at the end of his article, and had shown it, he thought, to be quite untenable even on the secondhand data then known to him, not having found it possible at that time to obtain access to the original of Rougnon's letter to M. Lorry, dated 1768. The letter, indeed, was rather difficult to procure even in France, as there were only one or two copies of it known to be in existence. But in a conversation he had in Paris with his friend Dr. Lereboullet he managed to interest him in the subject so far that he was kind enough not only to consult the original for him, but to have an extract made of all that was important in it as bearing on this question.

Professor Gairdner added that, according to Dr. Lereboullet's judgment, as well as his own, there was no trace of anything like a clinical description of angina pectoris in M. Rougnon's letter, and that the one case of unexpected death he referred to gave no evidence of its being due to that cause. It was impossible to suppose that of the very numerous authorities that had affirmed this claim on behalf of M. Rougnon even one could have intelligibly read the single document on which the claim had been represented as resting. It was quite certain, as was shown long ago by Parry, that Morgagni in 1761 did describe an individual case of angina pectoris, and it was equally certain, he thought, that M. Rougnon in 1768 did not; and yet both in France and Germany the latter had been generally accredited with being the first to make the discovery.

Dr. James Ross, of Manchester, likewise wrote regretting his inability to take part personally in the discussion, as he desired to present a theory to account for the distribution of the pains in angina and in other visceral diseases. He stated that his theory was that when a viscus was diseased there was local pain which might be regarded as of splanchnic origin (præcordial pain in the case of the heart). In addition, the irritation was conducted to the portion of the spinal cord from which the viscus derived its splanchnic nerve, and thence spread in the grey matter of the posterior horns, whence by the law of excentric projection it was referred to the termination of the somatic nerves derived from the segment of the cord—the second and first dorsal in the case of the heart. This explained the pain shooting between the shoulders and down the inner side of the arm (second dorsal) to the elbow and the ulnar border of the forearm and hand and ulnar fingers (first dorsal). The left was the one most usually attacked, because the left ventricle was the diseased one in angina. Besides these pains, the irritation was conducted inwards along the afferent fibres of the pneumogastric and reflected outwards to branches of the fifth cranial nerve as vertical headache. The close association of the function of the pneumogastric and phrenic might give rise to other disorders. Some cases of angina were, he thought, attended with spasm of the diaphragm, a muscle which derived its nerves from the second and third cervical segments of the cord. Whenever the diaphragm was affected, whether it were by spasm or by inflammation, as in pleurisy, pericarditis, or abscess of the liver, the chief pain was referred to the shoulder and back of the neck, in the region of distribution of the second and third cervical nerves (sensory branches). He thought that this shoulder pain was present in some cases of angina. He followed Gaskell in believing that the splanchnic nerves (visceral) were derived from the cells of Clarke's vesicular column, and whenever these columns were absent in the cord here were no reflected pains.

March 2nd, 1891.

OBSERVATIONS ON ADDITIONAL CASES ILLUSTRATING HEPATIC SURGERY.

By J. Knowsley Thornton, M.C., President.

In November, 1887, and again in December, 1888, I had the honour to read before the Medical Society papers containing my experience up to date in hepatic surgery. On March 2nd, 1891, I communicated to the same Society my further experience in this very important branch of abdominal surgery. The work is so comparatively new, and so interesting both to the physician and surgeon, and the individual cases differ so much in detail, that I venture, as in my previous papers, to record them a little more fully than I should do if dealing with some of the commoner operations performed on abdominal organs. I will take, first, nine cases in which I diagnosed gall-stones as the cause of illness, and in seven I found and removed them, with complete cure of the patients. In the eighth case I missed the stone, the patient died, and the stone and much effused bile were found in the peritoneum after death. In the fifth case the obstruction proved to be hydatids, and they were successfully removed. I will, then, refer more briefly to seven other cases, which have a more or less important bearing upon the diagnosis of pathological conditions of the liver.

Case 1.—A young lady of 32, much crippled with chronic rheumatic arthritis, had lived in early life on marshy land in Roumania. There had been several illnesses occurring at intervals during several years, which had been diagnosed as due to gall-stones. From the description given by the patient, and from the marked renal and bladder symptoms which accompanied the attacks, I was very doubtful whether they were gall-stone or right renal colics, and when I came to examine the abdomen I was still more inclined to think the illness renal, for there was a rounded tender swelling projecting from under the false ribs on the right side in front, and giving an impulse right back into the loin ; and, though there was some crepitus over the usual site of the gall-bladder, there was no swelling there and no very marked tenderness. On February 28th, 1889, Mr. Murray gave chloroform, and, assisted by Mr. Malcolm and in the presence of Mr. Heath, I made a small incision over the swelling, but rather to its inner side, in case it should turn out to be the gall-bladder, and found it was that organ completely enclosed in matted omentum and intestines ; having aspirated and drawn off a

little bile-stained mucus, I packed it carefully round with carbolised sponges, and opened it by a small vertical incision, and extracted two large stones with my sponge forceps and some *débris* with a lithotomy scoop. The adhesions made the examination of the right kidney very difficult, but it was certainly large and hard, and I greatly feared she might have renal trouble as well. I sutured the edges of the gall-bladder into the abdominal incision in the usual way and drained it, and she made a perfect recovery, all discharge ceasing on the fifteenth day. She has been in much better health in every way since the operation, and the kidney has given no further trouble; so I hope the renal symptoms were due to its adhesion to the inflamed mass formed by gall-bladder, intestines, &c. She has recently had a slight attack of pain, but I think from the description it was merely an ordinary "bilious attack."

Case 2.—Hospital patient, single, 37. Operation, March 16th, 1889. Symptoms for a year, two severe attacks of colic. After the second she was seen by Dr. Goodhart, who diagnosed gall-stones. She had never been jaundiced, but the stools had been pale ever since the first attack. Usual incision; healthy gall-bladder, with no adhesions; two good-sized stones in common duct. After aspirating and opening the gall-bladder, I found it impossible to dilate the cystic duct, which had closed behind the stones. I therefore needled them into pieces by passing a fine needle through the wall of the duct, and then further crushed the fragments with a pair of nasal polypus forceps, the blades being protected by red rubber tubing drawn over them. The gall-bladder had been so much injured in the attempts at dilatation that I decided to remove it. The liver was large and congested, and much in the way, and I found the operation much more difficult than in the case described to the Society in my last communication. After the operation she was distinctly jaundiced, and for eight or nine days there was a good deal of bile in the urine, but the first motion passed was brown, and the fragments of stone began to pass on the eleventh day. She made a good recovery, and went home well in a month.

Case 3.—Single lady, aged 40. Attacks of pain and indigestion every three or four months for the last six years. For the last two years pain has been much more severe and prolonged in the region of the gall-bladder, with eructations, sense of constriction round the chest, nausea, and vomiting. On March 24th, 1889, very severe attack; jaundice on 29th. April 24th, very severe pain for three days. June 2nd, succession of four attacks of severe pain, jaundice persisting since it first appeared. I saw her in consultation with Mr. Propert, and found her deeply jaundiced; small, quick pulse; temperature over 102°; great pain over the common duct. I diagnosed stone impacted in the common duct, and, looking to her very serious condition, urged immediate operation, and operated the same afternoon, Mr. Murray giving chloroform, Mr. Malcolm assisting, and Mr. Propert being present. I removed a large angular stone from the cystic duct after breaking it up, and found another impacted in the common duct just below the entrance of the cystic duct. Failing either to reach it through the cystic duct or to break it up, as in the last case, I incised the common duct, and needled the stone into fragments, which, however, I was unable to extract through any reasonable opening in the duct wall. I therefore left most of the fragments in the duct, carefully suturing the opening over them. I then sewed the edges

of the open gall-bladder into the external wound, and drained it in the usual way, passing a glass tube also into the peritoneal pouch below the liver. The highest temperature after the operation was 100·2°, pulse 72, and after this normal. All discharge ceased on the sixteenth day, and she was up on the nineteenth; all jaundice gone and urine and fæces normal, and she rapidly recovered perfect health.

Case 4.—Married lady, aged 35, mother of five children. Lump just below liver and pain noticed six years ago, just after birth of last child. Several attacks of pain after this; no jaundice. Came to me in July, 1889, during attack; the last previous attack had occurred a year before. I could distinctly feel a number of stones in the gall-bladder, and advised their removal. Operation, July 31st, 1889. A simple cholecystotomy, gall-bladder sewn into abdominal incision and drained; bile-stained mucus discharged very freely at first; entirely ceased in a fortnight. Temperature once rose to 101°; was normal on the seventh day. Excellent recovery, and remains well.

Case 5.—Hospital patient, single woman, aged 32, sent to me by Dr. Jack, of Emperor's Gate, was seized with sudden severe pain in the region of the gall-bladder during sleep, in May, 1889. The pain continued all next day, recurring in severe paroxysms; two days later she was jaundiced and the urine was yellow. This attack entirely passed off with a simple purge. On October 11th a similar attack, in which she was attended by Dr. Jack, who found a tumour in the region of the gall-bladder and sent her to me, having first aspirated the tumour and drawn off two pints of bile-stained fluid. The tumour refilled rapidly. On November 11th, 1889, I cut down upon the tumour, and, having removed two pints and a half of bilious fluid with the aspirator, opened it and removed a quantity of hydatids, and found that it was the gall-bladder. There were many adhesions all round it, and I removed a separate hydatid cyst from the omentum. As there was fluid in the pelvis, I made a counter opening over the pubes and drained the peritoneum through it by a glass tube. The gall-bladder was sutured into the abdominal wound and drained. The temperature rose to 102° on the first two evenings, but was normal on the fifth day. The pelvic tube was removed in thirty-six hours. The gall-bladder discharged for twenty-two days. Perfect recovery.

Case 6.—Married woman, aged 43, mother of twelve children, youngest 4, was sent to me in same month as above by Dr. Maurice, of Marlborough. She was much emaciated and deeply jaundiced, with that peculiar earthy bluish tinge which so often accompanies malignant disease. Indefinite swelling and dulness in region of gall-bladder, but more to left than usual. Liver large and hard, and obvious matting of parts below it; no ascites. First attack nine months before. Severe pain, which continued off and on for two weeks; did not notice her stools or urine. At end of two weeks violent vomiting, complete anorexia, and green jaundice. On admission, colour as above, tongue clean and moist, fair pulse, small hard glands in both groins, pelvic organs normal. I diagnosed impacted gall-stone and advised operation, but could not help being a little anxious, lest it should prove to be associated with malignant disease. I operated on November 28th, 1889, in presence of Dr. Maurice, Sir Spencer Wells, and many other visitors. I found the liver so large that

the gall-bladeer and ducts were quite covered, much adhesion of omentum and intestines, a large oval stone impacted in common duct, and, failing to define the gall-bladder accurately, and feeling sure that it would be impossible to get at the stone through a contracted cystic duct, I decided to incise the common duct at once and so remove it. I was much hampered by the large liver, and had to work entirely by touch deep in the abdomen, guiding the knife on the left index finger. The first incision was followed by such a rush of dark venous blood that I feared for the vena cava, but, having introduced a small Ferguson's glass speculum, I saw that it proceeded from a vein in the adherent omentum, and this I easily secured. I had great difficulty in loosening the stone, which was firmly adherent to the lining membrane of the duct over a great part of its surface. After removing it I could see the termination of the duct in the duodenum quite distinctly through the speculum. I closed the opening in the duct by six fine interrupted points of silk suture, and a continuous one over all, using the omentum to strengthen the closure as I had done in the previous case of duct incision. I drained the peritoneum by a glass tube. She suffered very seriously from shock, and the first urine was half albumen; there was none before the operation. She quickly rallied, however. The temperature never rose above 100·6°, with a pulse of 100, and both were normal on the fifth day. All drainage was over on the sixth day; there was a natural action of the bowels on the fifth day; the urine began to improve in colour on the third day, but the stools were clay-coloured till the eighth day. The wound was healed in a fortnight, and she left the hospital in three weeks quite well, but still slightly jaundiced. Dr. Williamson, of Ludgershall, writes that the jaundice lasted about five weeks after her return home, that she has grown stout, and is quite well and able to do all her house work.

Case 7 was that of a single lady, aged 36, whom I saw in consultation with my friend Mr. Hewer, at Highbury. The patient had long suffered from inactive liver, with sallow complexion, and attacks of pain in the right hypochondrium, suggesting the presence of gall-stones. For the last six months the attacks had clearly been due to attempted passage of gall-stones; the pain became very violent, and was accompanied by vomiting, jaundice, and rapid emaciation. Sir Andrew Clark, and afterwards Dr. George Harley, saw her, and the latter made a prolonged attempt to massage the stones into the intestine, but without effecting any amelioration of the symptoms. When I saw her with Mr. Hewer she was deeply jaundiced, with raised temperature and quick pulse, and we agreed that if relief was to be given there was no time to lose, and I operated on the next day, May 3rd, 1889. I found two stones impacted one above the other in the common duct. The gall-blader was so shrunken that it was difficult to recognise it, and I at once decided to open the common duct, remove the stones, and suture the opening. This procedure I carried out with much difficulty, owing to the matting of parts by adhesions; and not being very well satisfied with the security of the suturing of the duct, I passed a rubber tube into the pouch, at the bottom of which it lay, and brought it out through the upper part of the abdominal incision, and I also passed a glass tube into the pouch of Douglas through a counter opening above the pubes. The latter was hardly required, but a very heavy discharge of bile-stained serum flowed from the rubber tube for many days after the operation, showing that my fears as to the security of the duct sutures were well founded. She recovered rapidly,

without a single bad symptom, and Mr. Hewer reports on February 13th, 1891 : " Perfectly well, good complexion, and takes her food without any inconvenience."

Case 8 was that of a young married woman, aged 34. She had suffered for a long time with attacks of pain round the back and in neighbourhood of liver, which lasted a few days, and then entirely passed off. In January of last year she noticed a lump in the situation of the the gall-bladder which varied in size. In February she had a severe attack of pain, with tympanites and severe vomiting. The lump was said at this time to be irregular in outline, and so mobile that it was thought to be in the omentum, and of a malignant nature. This diagnosis was strengthened by gradual emaciation with constant pain, and by her mother having died of internal cancer. When I first saw her I found a smooth oval tumour about as large as a turkey's egg, and with all the characters of a distended gall-bladder. She went away for a time, and when I saw her again I found a much smaller, harder, and more sensitive swelling, and this confirmed my diagnosis ; but I feared it might be a case of malignant obstruction rather than stone. I operated on May 20th, 1890, and found a very hard and thickened gall-bladder, and laid it open and hunted carefully, but could find no stone. The adhesions round it were so firm that it was difficult properly to explore the ducts from their peritoneal surface, and the thick wall of the gall-bladder cut so easily with the sutures that I determined not to attempt to fasten it into the incision, but to suture it carefully and drop it in, this, as it proved, most unfortunate decision being aided by the unusually small amount of bile or mucus which escaped from the bladder during the necessary manipulations of the operation. The patient did badly from the first, and died on the third day with septic symptoms. An examination after death showed that a large quantity of bilious fluid had escaped into the peritoneum through the giving way of the sutures in the gall-bladder wall, and close to the opening lay a small gall-stone free in the peritoneal cavity—a remarkably small one to have caused so much trouble and such great thickening of the gall-bladder ; but no other disease could be found to account for the mischief.

Case 9.—A single lady, aged 34, whom I saw in consultation with Mr. Edgcombe Venning, to whose kind care of the case after operation I am indebted for being able to add it to the list of successes. I operated on May 21st, the day following the last case. Attacks of pain began in 1875, and were always sudden and spasmodic ; sometimes one or two years would pass without an attack. The attack which led to my being called in began in the previous November, the attacks of pain and vomiting continuing at intervals till I saw her. On opening the abdomen I found everything so matted together that I at first mistook the head of the pancreas for a thickened gall-bladder. Finding my mistake, I proceeded to carefully make my way through some fresh adhesions between the stomach, the omentum, and the right lobe of the liver, and at last found the gall-bladder, deeply placed and packed with gall-stones. In searching for it I opened a small abscess under the edge of the liver, and let out about an ounce of thick pus, and cleared away some cheesy material. The clearing out of the stones was long and difficult, and required the aid of various scoops and forceps ; when I had finished and washed it out I found that, as in Case 7, it was impossible either to remove it, suture it, or suture it

into the abdominal opening, and I therefore merely passed a rubber tube to the bottom of the gall-bladder, and brought it out through the abdominal wound ; this of course leaves the opened gall-bladder open to the general peritoneum, and I therefore made a counter opening above the pubes, and drained the pouch of Douglas also with a glass tube. The procedure of leaving the gall-bladder open seems at first sight very dangerous, but these two cases prove that with due care in the arrangement of the tube, and with the auxiliary drainage of the pelvis, it may, when necessary, be safely resorted to. In both cases the wound in the gall-bladder was slow in closing, as one would expect from its being so much wider open than when sutured into the incision ; but there was no evidence of any serious leakage from the open gall-bladder into the peritoneum, though the discharge of bile-stained mucus through the rubber tube was very free from the first. I suppose that when the abdominal incision is sutured, and the abdomen well strapped up, the adherent viscera round the gall-bladder or duct, as the case may be, press up to the parietal peritoneum, and the fluid finds it easier to escape through the open tube than to spread into the peritoneum between the surfaces of these structures. Then in a very short time a firm wall all round the tube is formed by the adhesion of neighbouring surfaces. This patient also made an excellent recovery, and on recent inquiry writes that she continues "a walking advertisement" for surgery.

Case 10.—This case was one of a large hydatid cyst of the liver, which I saw in consultation with Dr. Ransford, and which had been thought to be some disease of a more serious kind. I felt perfectly certain of my diagnosis, and verified it by clearing out a large quantity of hydatids and semi-purulent fluid, then washing the sac well out with iodine and water, and draining it with two large rubber tubes. It was long in closing, and after it had closed the patient began to have recurrence of shivering and fever, and at last such distinct enlargement of the right lobe of the liver that I reopened a small spot in the cicatrix, and punctured with a long fine trochar, expecting to find recurrence of suppuration. Nothing but dark blood came, though I punctured deeply in various directions. The result of this phlebotomy was an immediate drop of the temperature ; but in a very few days it rose again, and all the old symptoms—chilliness, nausea, anorexia, &c.—returned. After a time, however, the patient gradually improved, and is now, I think, going to get well without further surgery. The cause of the serious recurrence of symptoms remains a mystery, as there has certainly been no escape of pus anywhere. I think it is possible there may have been some deep effusion of serum, which caused the symptoms by setting up tension and obstructing the hepatic circulation, and that this has been gradually reabsorbed.

During the time that has passed while completing the above cases I have also performed five exploratory operations in connection with disease of the liver, a large proportion of explorations in proportion to the number of completed cases. But this must always be so when one is dealing with a comparatively new departure in surgery, and especially in a field in which positive diagnosis is so difficult as in the one we are considering. Two of the cases turned out to be, as I feared they would, malignant

disease. In the first the exact origin of the disease remained obscure after the exploration, owing to the matting of parts, and the patient, an elderly gentleman, in a very low state at the time, died of complete suppression of urine. I did not myself think this was a gall-stone case, but he had many of the symptoms of impacted gall-stone, including jaundice, and, as there was no free fluid in the peritoneum and no distinct evidence of malignant disease, I thought, with Dr. Broadbent, that it was a proper case for exploration.

The next case was that of a lady, who had been till shortly before I saw her (also with Dr. Broadbent) in the best of health. There was a small, hard, round, mobile body in exactly the situation of the gall-bladder, and here again there was no free fluid, and she herself was very anxious to have the diagnosis made positive by exploratory incision. I found that the hard body named above was a little malignant mass projecting from the right lobe of the liver, with more or less of a pedicle, and that there were other masses embedded in the liver, so I at once closed the incision, and she was happily none the worse for it, and lived for some months.

The next case was that of a young lady whom I saw with Mr. Cripps Lawrence, a case with a long history of pain, and of attacks like gall-stone colic, and I certainly expected to find gall-stones, but there were none, and I could find nothing very definite to account for the pain. I found, however, a small hard body, which appeared to me to be a new growth, but I could not define its exact place of origin, and I closed the incision with some fear that this might eventually turn out to be a case of malignant disease, but the long history before and her health since seem to contra-indicate it. Mr. Lawrence writes on January 29th that the patient gained flesh and strength after the operation, and was free from pain for some time, but had relapses in July and October, the latter being somewhat severe and prolonged. Still she has gained on the whole in general health, and there seems no evidence of any serious organic disease. After my experience with Case 8, an unsuccessful exploration of this kind, with recurrence of symptoms, naturally made me wonder whether I could possibly have missed a small stone.

The fourth exploratory operation was performed upon another case of Mr. Venning's, a young gentleman who had knocked about the world a great deal, and had often been much exposed in travelling, hunting, and shooting expeditions. He had many of the symptoms of gall-stone, and a very distinct hard and tender swelling in the region of the gall-bladder; and after careful consideration, as he was losing health and suffering a good deal of pain and inconvenience, we decided to explore the swelling. It turned out to be a hard, almost calcareous mass, projecting from the right lobe of the liver just over the gall-bladder, and on puncture it was gritty and yielded just a trace of cheesy material on the needle. Below this and on the under surface of the liver were a number of pinkish, somewhat translucent nodules, which certainly looked very like malignant disease. I was a little inclined to regard the calcareous mass as an old hydatid; but on careful consideration of the case in the light thrown on

it by the exploration, we were rather in favour of the whole thing being gummatous, and he admitted that he had had syphilis. This view proved to be correct, and he got quite well with large doses of iodide of potassium. In connexion with this case I may mention another (also a young gentleman) in which I was asked to perform paracentesis abdominis, the idea being that he was suffering from hepatic dropsy resulting from cirrhosis. After the removal of the ascitic fluid I found a greatly enlarged nodular liver—some of the nodules hard, some soft—and I suggested that they might be gummata. The case is not concluded, but he has improved in health under the use of large doses of the iodide, and the liver has got markedly smoother, so that I think we are on the right track.

The fifth case of exploratory incision I saw in consultation with Dr. Ord. The lady (middle aged) was a confirmed invalid and constantly attended by two nurses. She had passed some very large gall-stones, and another was believed to be present in her gall-bladder, causing much pain. I could not convince myself by examination or from the symptoms and history that it was a case of gall-stone colic, but I quite agreed with Dr. Ord that it was a proper case for exploration. I found the gall-bladder and its ducts and surroundings healthy, but some matting of parts from previous inflammation. I simply divided the adhesions and closed the incision, and the patient lost her pain and is now in good health. The case appears to be identical with that of the Irish lady which I gave in my previous paper ; both were exceedingly ill, and both were restored to health by the exploration and separation of some adhesions.

I have recently had another case with Dr. Ord and Mr. Turner, an officer aged 63, with an enormous hepatic abscess ; the surface of the liver was slightly adherent to the parietes, so that I was able to open it without exposing the peritoneal cavity, and, in order to avoid all risk of the liver tearing away as the abscess emptied, I put a fine suture through the upper and lower angles of my abdominal incision, and the corresponding angles of the incision into the liver. The abscess had been explored the day before I saw him with a hypodermic syringe, so I could not be quite certain of asepsis, and therefore thoroughly washed out the cavity with a strong solution of iodine and water. Discharge was very free for the first week, then steadily diminished for the next, and by the end of the fortnight was practically serous, so that the dressings could be left for two or three days undisturbed, and now, at the end of three weeks, the cavity has practically closed, and the patient has gained extraordinarily in the short time in health and strength, a remarkably quick result in a patient of this age, and with an abscess holding several pints of pus.

I have given in more or less detail notes of sixteen cases in which I have operated. Let us now consider the lessons which hese cases teach. In ten I expected to find gall-stones ; I found them in seven. I diagnosed another case correctly, but unfortunately failed to find the gall-stone, though it was there ; and in another case the disease was in the gall-bladder, and the symptoms were due to obstruction of the ducts, but it was hydatids, and not

gall-stones. In a tenth case I diagnosed hydatids of the liver, and the operation proved that my diagnosis was correct. In three other cases I explored, not expecting to find gall-stones: in two I found what I expected, malignant disease, but I cannot claim accurate diagnosis, because I was in doubt; in the third case I did not think I should find a gall-stone, and I found merely some adhesions, the result of previous trouble during the passage of large gall-stones, but the division of these cured the patient, so that though the diagnosis was not perfect the result was good. Three cases remain. In one the diagnosis of gall-stones was not justified by the exploration, and the patient is little, if any, better for the operation; in another, no gall-stone was found, but the exploration led to a new diagnosis, which was proved by successful medicinal treatment to be the correct one; and in the last case the diagnosis had been made for me by Dr. Ord and Mr. Turner, and verified by puncture before I operated, and the result of operation has been eminently satisfactory. I think, then, on the whole series the results are good, and amply justify the operative procedures. At the same time there is sufficient uncertainty and failure to show that it is to a more perfect diagnosis that our chief attention must be directed. I believe that, as experience is gained by more frequent exploration in doubtful cases, we shall advance in certainty of diagnosis; but I think it is too much to hope that we shall ever attain certainty in all cases. With regard to gall-stones, I think the chief points to be noted are the sudden nature of the onset of pain and its equally sudden departure, the way in which it travels round the body and through into the back at the angle of the scapula, and the sense of constriction round the region of the diaphragm. The presence of a mobile pear-shaped tumour in the situation of the gall-bladder, or to one or other side of that situation, which rises and falls with respiration, variations in size and tension of this swelling may make diagnosis pretty certain. The above symptoms, being suddenly complicated by jaundice, especially if a swelling previously present disappears, make it pretty certain that the stone has passed through the cystic duct and become impacted in the common duct.

It is obvious from two of the cases I have given that the absence of ascitic fluid cannot be relied upon as a differential diagnostic sign between malignant disease and stone in either duct, and we must always remember that gall-stones may be present, and actually

occasion a course of symptoms along with malignant disease, also causing obstruction of the ducts; for most interesting notes of such a case I am indebted to my friend Dr. Barlow. My absence from town alone prevented my seeing this case in consultation with Sir William Savory and himself, and I think it is very likely that the symptoms would have led me to urge operation; but the necropsy showed that, though the removal of the stone might have given some relief, the case would still have terminated fatally from malignant disease of the pancreas obstructing the common duct.

The position of the swelling when the cystic duct is obstructed varies so much in different cases that it has but little diagnostic value; and in cases in which there has been much inflammation, matting the parts above the gall-bladder, neighbouring organs, especially the kidney, are so liable to sympathise that the greatest care is necessary not to be led astray into considering the renal symptoms as the essential feature of the case. My first case in the present series illustrates this well; it also shows how a patient suffering from a serious chronic disease like chronic rheumatic arthritis may have all her symptoms relieved and her general health improved by the removal of a serious source of local irritation.

Case 2 illustrates the success which may be obtained by merely needling up the stones in the common duct, and leaving the fragments to find their own way into the bowel. To obtain success in needling, however, I think it is essential that the stones be not too large, and that they have some mobility in the duct. Had I needled these stones at once the patient would have been saved the serious risk of a difficult cholecystectomy, though, now she is well, I think she is more likely to keep so without her gall-bladder.

Case 3 also illustrates what may be done with needling even after opening of the duct when we have to deal with a large adherent stone; it also shows, what I believe was unknown before, that it is possible to operate directly upon the common duct and suture it securely even though fragments are left in it. This case and No. 6 (that of Dr. Maurice) also show that prolonged jaundice and a great state of emaciation and weakness are no bar to the most rapid and complete recovery. Prolonged jaundice has been said to interfere with success, but I have never found it make any difference.

Case 5 illustrates the difficulty of differential diagnosis between gall-stones and hydatids when the latter are confined to the gall-bladder.

Cases 6 and 7 show the advisability of at once opening the common duct when large stones are found to be impacted in it; and the intimate adhesion of the stones to the lining membrane of the duct shows not only the futility, but extreme danger, of attempting to force them down by external manipulation, as proposed by Dr. Harley, and practised by him in Case 7 with marked increase of all the serious symptoms.

Case 8 shows how dangerous it is to be influenced by previous history or the opinion of others. I made a correct diagnosis, but could not divest my mind of the possibility of malignant disease; the result of this bias was two serious mistakes in my procedure:—1. I did not prolong my search for the stone sufficiently. 2. I feared a malignant fistula if I sewed the gall-bladder into the abdominal incision. Hence my patient lost her life, and I spoilt what would have otherwise been a completely successful record. I have sufficiently commented on the remarkable fact shown in Cases 7 and 9, that the gall-bladder may be left open in the peritoneum or the duct imperfectly closed if efficient drainage is provided, without any serious complication resulting, and without the escape of bile into the general cavity of the peritoneum. The counter opening above the pubes and the glass tube in the pouch of Douglas I commend as a great safeguard in all doubtful cases, and with perfect asepsis I believe that it in no way increases the risk.

The above cases, together with those already laid before the Society, complete the record of my experience in hepatic surgery up to the end of January, 1891, and I trust they may be of value to the future of this very interesting branch of abdominal surgery.

March 16th, 1891.

FATAL CASE OF LITHOLAPAXY.

By Edmund Owen, F.R.C.S.

Mr. Edmund Owen reported a fatal case of rupture of a boy's bladder, which accident occurred during the performance of the operation of litholapaxy. The patient was an unhealthy child of something less than 5 years, who for two months had been troubled with frequent and painful micturition, the urine being thick and offensive. He had been taken to a large hospital, where, the mother said, circumcision had been advised; but it was also suggested that the boy was not at that time in a fit state to be operated on. He then came under Mr. Owen's care, and, on sounding him, a small stone was discovered. Slight bleeding followed this examination. The boy was put to bed and fed up for twelve days, in order that his strength might be so much improved that he could the better stand the operation of litholapaxy after the manner of Surgeon-Major Keegan. At the end of this time chloroform was administered, a No. 6 lithotrite was introduced, and the stone was effectually and efficiently dealt with. The bladder was then washed out, and the fragments were removed by a No. 9 evacuation tube. The operation was thus practically at an end. A final washing was being performed, however, when an ominous gurgling was heard, and an unmistakable wave ascended behind the anterior abdominal wall. All resistance, moreover, to the flow from the evacuator had ceased; and, on detaching the wash-bottle, only a trifling amount of water escaped by the tube. There could be no doubt that the bladder had given way, and that the best chance of the boy's recovery probably lay in opening the abdomen and sewing up the rent. Accordingly, a 3-inch incision was made in the linea alba above the pubes. The peritoneum was opened, and a search was made for the collapsed bladder in the depths of the pelvis, nothing being seen of the bladder until the peritoneum had been traversed. The rent, however, was eventually discovered in front of the peritoneum, and at the root of the urachus. It was also made out that the irrigation-fluid which had escaped from the bladder had infiltrated itself along the subperitoneal connective

tissue, none of it having entered the peritoneal cavity. The rent in the bladder—which was about one-eighth of an inch long—was sutured, and the abdominal wound was closed. It was deemed expedient to secure absolute rest for the bladder by a perineal drain. A lateral incision was therefore made through the prostate, and on passing the finger through it into the bladder, a few insignificant fragments of stone, which were to have been removed by the final washing, were detected. The boy died of collapse in about eight hours. Mr. Owen attributed the calamity to the fact that in the temporary absence of a wash-bottle of a size appropriate to childhood he was using one designed for the adult. He had no extenuating circumstances to urge. The case had been a great grief to him, and he had come to the conclusion that the only way in which he could atone for the misadventure, in however slight a degree, was by openly reporting it. Perchance his experience, thus being placed on record, might be the means of averting a similar catastrophe at the hands of others.

LITHOLAPAXY IN CHILDREN.

By Surgeon-Major D. F. Keegan, M.D.

My first duty is to thank you for the honour you have done me by requesting me to exhibit to your Society this evening my collection of calculi removed from the bladders of male children and boys by litholapaxy on Bigelow's method of lithotrity. The collection which I show you consists of the *débris* of 117 calculi, all removed at *one operation*. When I left India in May last, I had myself performed 125 litholapaxies in male children and boys below 15 years of age. My collection is therefore deficient by eight specimens. The *débris* of the first two calculi which I removed from male children by Bigelow's method of lithotrity—now nine years ago—were not weighed or put carefully away in bottles, and six specimens have either been lost altogether or inadvertently left behind me in India. In the 'Lancet,' of October 4th, 1890, I tabulated the main features of interest connected with each of these 125 litholapaxies. The ages of the boys on whom I operated varied from 1 year and 9 months to 14 years, the average age being 6·38 years. The average weight of the calculi removed

was 89·15 grains. The average number of days spent in hospital after operation was 4·6, and the percentage of mortality was 3·2. The four deaths were not the result of any failure or accident during the operation, but were due in each case to organic disease of the kidneys. There was but one recurrence of stone amongst the 125 cases. Recurrence of stone does not follow litholapaxy in male children any oftener than it does lateral lithotomy, provided litholapaxy be properly performed. And when I say properly performed I mean when no *débris* is left behind in the bladder after the operation. This absence of recurrence of stone after litholapaxy in boys is what we might have anticipated, for the bladders of boys are seldom hypertrophied, sacculated, or fasciculated, and boys cannot labour under enlargement of the prostate. And we know from experience that these are the diseased conditions which mainly conduce to recurrence of calculi in the aged after litholapaxy.

I have for some years advocated the adoption of litholapaxy in male children and boys in preference to lateral, median, and supra-pubic lithotomy, principally for two reasons: first, on account of rapidity of cure, and, secondly, because litholapaxy is free from the objections and dangers inherent in all surgical operations in which the knife is used. The more I practise litholapaxy in boys the more favourably am I impressed with its superiority over its three rivals when once the surgeon has become familiar with the use of the lithotrite, and he has acquired the *tactus eruditus* so necessary to success in this special surgical operation. But, gentlemen, remember that I do not say that the surgeon who meets with cases of stone in the bladder at only fitful intervals in his career, and who has had very little practice in what I may call the instrumental treatment of urinary diseases, will be acting wisely in attempting to perform litholapaxy on a young male child the subject of a calculus of considerable size and hardness. Such a surgeon, I think, will act more wisely if he adhere to the lateral or supra-pubic methods of procedure. But there can be no manner of doubt that litholapaxy, as applied to the treatment of the vast majority of stones one meets with in male children and boys, is a superior operation to lateral, median, or supra-pubic lithotomy, provided the surgeon has at hand a supply of the requisite instruments, and that he knows how to use them. With my present experience, I should no more dream of cutting a boy of 8 years

of age for a stone weighing a drachm than I should think of cutting an old man of 70 for a stone of that weight.

In addition to the collection of calculi which I have the honour of showing you, I have brought with me a collection of lithotrites and evacuating catheters, or canulæ, and one or two aspirators. Since my return from India I have spent three or four months of my furlough in London, and, from what I have seen, I have come to the conclusion that litholapaxy, as applied to the treatment of calculi in male children and boys, is not at present getting fair play at many of the hospitals in this great centre of medical education. And when I say that it is not getting fair play I mean that surgeons are very frequently handicapped in the performance of this delicate operation by the paucity of those instruments which are absolutely necessary to its successful accomplishment. No surgeon, in my opinion, should set about attempting to perform a litholapaxy on a boy trusting to one lithotrite and one evacuating catheter, for a lithotrite or a canula which will pass readily enough into a boy's bladder at the beginning of the operation may not do so towards its termination, when the urethra has perchance become swollen and congested by the frequent passage of instruments. And therefore I say that a surgeon is neither giving himself nor his little patient a fair chance if he trusts to one lithotrite and one canula. 'Tis true, no doubt, that a supply of lithotrites and evacuating catheters costs a good deal of money; but if they are well made and properly taken care of they will last a lifetime. The collection of instruments which you see on the table, all made by Weiss and Son, would cost about £47. It consists of five lithotrites, Nos. 4½, 5, 6, 7, and 8, and five evacuating catheters, Nos. 5, 6, 7, 8, and 10, and two aspirators. I do not say that it is absolutely necessary that a surgeon should have at hand as large a supply of instruments as you see before you every time he sets about crushing a stone in a boy's bladder. Two or three lithotrites and three canulæ would probably suffice; but, the more he has, the better for his patient and himself. You will perceive that the lithotrites are all fully fenestrated, and that the evacuating catheters or canulæ are all fitted with serviceable stylets. I assign great importance to using only fully fenestrated lithotrites in performing litholapaxy in boys, for this pattern of lithotrite cannot become clogged with *débris*. Clogging of the blades of a lithotrite with *débris* may prove a most dangerous complication in performing

litholapaxy on a male child, because clogging has the effect of increasing the size or number of a lithotrite, say, for example, from a No. 7 to a No. 8. And as in performing litholapaxy in male children we are dealing with urethræ in which there is not much spare room left to work in, the effect of increasing the size or number of the lithotrite whilst it is in the bladder may be such that in withdrawing it from the bladder the urethra may get unduly stretched or ruptured. Now such an accident or complication cannot happen if we use only fully fenestrated lithotrites. And here I would state that experience has taught me that the narrowest part of the urethra in a male child, if we except the meatus, will generally be found not at its membranous portion, but at the junction of the penile with the scrotal portion of the urethra, *i.e.*, about $2\frac{1}{2}$ to 3 inches from the meatus. It is very important that the evacuating catheters or canulæ should be fitted with stylets, first, to render their passage into the bladder more easy, and, secondly, for the purpose of dislodging any fragments of stone which may become impacted in the eye of the canula. Before seizing the stone for the first time with the lithotrite, the child's bladder should be emptied of urine, and then 2 or 3 ounces of water, at blood heat, should be thrown in by the aspirator. When the stone has been crushed and the lithotrite withdrawn, the canula, fitted with a stylet, is introduced into the bladder. The stylet is then withdrawn, and nearly all the water in the bladder allowed to drain off. The aspirator, filled with water at blood heat, is now attached to the canula, and while this is being done it is a wise plan to make your assistant press gently over the bladder for the purpose of preventing air entering that viscus. The aspirator should be worked gently, and not in a spasmodic fashion.

If these precautions are taken there can never arise any risk of over-distension of the bladder. The canula should never be withdrawn from the bladder while it is attached to the aspirator, and the stylet should be run home before the canula is removed from the bladder, for one can never be perfectly certain that a fragment of stone may not be impacted in the eye of the canula, which fragment would lacerate the urethra on its withdrawal from the bladder. If possible, not a grain of *débris* should be left behind in the bladder, and five or six minutes spent at the end of the operation in searching for the last particles of stone will be well and

profitably expended. It frequently happens that some small fragments lie close to the neck of the bladder, and are not influenced by the current of water thrown in by the aspirator. If the right index finger be introduced into the rectum, these fragments will frequently be felt lying between the convexity of the canula and the bladder wall, and by a to-and-fro movement of the finger can be readily removed from this position into the middle of the floor of the bladder, and are then carried into the current of water thrown in by the aspirator. The height of the operating table should be such that the surgeon will not be obliged to stoop over his patient. If during a prolonged operation he be compelled to stoop, he soon gets fatigued, and then he is tempted to hurry on his work, and may not be as careful as he should be to search for the last fragments of stone lying in the bladder. All haste and hurry is much to be deprecated in performing lithotapaxy in male children, and a light hand is almost essential to success. Anything approaching to forcible introduction of lithotrites or canulæ into the bladder is fraught with danger. The lithotrites which I exhibit, small though they may appear, are capable of crushing calculi of considerable size and hardness, and to illustrate their power I shall very briefly sketch the salient points connected with the operation of crushing a few out of the large number of stones which you see before you.

Case 1.—A Hindu male child, named Roopla, aged 2 years and 2 months, was admitted into the Indore Hospital on January 27th, 1890. Next day I crushed a uric acid stone weighing 65 grains. The operation lasted thirty-two minutes. I introduced the $4\frac{1}{2}$ lithotrite once and No. 6 lithotrite twice. Evacuating catheters Nos. 7 and 8 were used twice each. Although the boy was so young I had not the slightest difficulty in passing a No. 8 catheter; nor had I occasion to slit up the meatus. This child had a larger urethra than I have sometimes met with in children of 5 or 6 years of age.

January 29th (next day): Cries when passing water; micturated five times during the twenty-four hours; urine clear.—January 30th: Cries less when passing water; passed urine four times in twenty-four hours.—January 31st: Does not cry when passing water and passes it freely. Quite well; allowed to go home.

Case 2.—A Hindu boy, named Kaniram, admitted January 15th, 1890, suffering from stone for fourteen months. Same day I crushed a stone weighing 110 grains, composed of oxalate of lime, urates, and phosphates. The operation lasted forty-six minutes. Lithotrite No. 6 introduced three times, and lithotrite No. 7 twice. Canula No. 8 used six times, and canula No. 9 three times.—Next day, the 16th: Morning temperature, 98·2°. Burning sensation in passing water much less than before opera-

tion ; had seven micturitions during eighteen hours ; urine brownish. The boy plays about. Evening temperature 98·4°.—17th : Morning temperatuere, 98°. No burning sensation in passing water ; passed urine eight times in twenty-four hours.—18th : Doing well ; no pain.—19th : Went home.

Case 3.—Kekdyo, a Hindu boy, aged 9 years, admitted into hospital on the 28th November, 1887, with stone symptoms, which had existed for three years. Next day I crushed his stone, which weighed 535 grains, in sixty-six minutes. The exact chemical composition is written on the label. I sent 123 grains of the *débris* to Calcutta for chemical examination. The steps of the operation were as follows :—Found the stone very large, too large for No. 7 lithotrite ; slit up meatus, thinking that I might be able to pass No. 9½ lithotrite, but found that this was not possible. Then passed a No. 7½ lithotrite and began to chip away at the rim of the stone and eventually succeeded in reducing the stone to the capacity of the lithotrite, and cracked the stone across. When nearing the close of the operation on withdrawal of the lithotrite found it grasped very firmly at the junction of the scrotal and penile portion of urethra. It, however, came through all right, and then I found that the cause of the obstruction was explained by a small piece of *débris* having stuck in one of the notches on the convex portion of the male blade. Three days after operation I find the following note in my case book : "Doing well ; no urinary symptoms ; no rise of temperature ; passed urine five times during the past twenty-four hours ; boy has gone into the city of Indore to see the sights there."

Case 4.—A Mussulman boy, aged 10 years, named Niajali, was admitted into hospital on the 3rd August, 1889, with stone symptoms which he avowed had existed since his birth. The same day I crushed his stone, which weighed 202 grains, and which consisted of oxalate of lime and urates ; operation lasted seventy-seven minutes. I find the operation described as follows in my case book :—"The largest size lithotrite I was able to introduce was a No. 7½ ; the stone was extremely hard and defied the power of the lithotrite again and again. I eventually succeeded in breaking across the body of the stone by means of a No. 7 fully fenestrated lithotrite, which I think is more powerful then the No. 7½. The No. 7 lithotrite is a newer pattern of lithotrite and the pitch of the screw is higher. I find the following note in my case book three days after the operation :—"No rise of temperature ; no pain anywhere ; sleeps well ; walks about ; passed urine five times during the twenty-four hours."

This boy, although 10 years old, had a smaller urethra than many a boy of 5. I can well believe that he may have really suffered from stone all through his life, for I recorded a case in the 'Indian Medical Gazette,' of September, 1888, where I crushed a black mulberry stone, weighing only 32 grains, in a boy aged 11 years, and which stone had taken six years to reach this weight.

Case 5.—A Hindu boy, aged 13 years, named Dhemia, admitted into hospital on the 18th May, 1888. On the 20th May I crushed his stone, which weighed 606 grains, in one hour and forty minutes. The stone

was examined by the Professor of Chemistry in the Calcutta Medical College, and consists of 48·48 uric acid, and colouring matter and oxalate of lime, 51·52. The steps of the operation I find thus reported in my case book. "I first introduced a No. 7½ fully fenestrated lithotrite and readily seized the stone, but found it much too large and hard for such a lithotrite. I then slit up the meatus and introduced a No. 9½ fully fenestrated lithotrite, seized the stone again, but could make no impression on it; withdrew lithotrite and passed in readily a No. 10½ lithotrite; seized the stone but could not get the lithotrite to lock. I then began to seize the stone in different diameters and came to the conclusion that the stone was globular, and from the sensation conveyed to my fingers felt sure that it was studded over with rough knobs and thought that if I could seize two knobs not far apart I might break them off and so work into the body of the stone. After several ineffectual efforts I chipped off some of the knobs and eventually wore the stone down and got the lithotrite to lock. When locked I screwed the lithotrite up to its full power and held the stone firmly grasped between the blades, but I could make no impression upon it, and I feared to screw the lithotrite any harder lest I should break it. I therefore unscrewed the lithotrite and began to chip away again at the rim. I then withdrew the lithotrite and passed in a No. 10 evacuating catheter and worked the aspirator and found that I had knocked or chipped off a considerable quantity of *débris*, black in colour. I withdrew the catheter and again passed in the No. 10½ lithotrite and began the chipping process again. After some ten or fifteen minutes spent in this way I eventually seized the stone in such an axis that I was enabled to break off a considerable piece of it. This piece I broke up into smaller pieces and then directed my attention to the body of the stone, and eventually I had the satisfaction of breaking it across. On several occasions I was obliged to unscrew the lithotrite after having worked it up to its full power, for fear of breaking the instrument, and had I been rash I should certainly have done so. Having broken the main stone into small fragments I finished the operation by means of the No. 7 lithotrite. This boy never had a bad symptom after operation, and left the hospital perfectly well on the seventh day after admission.

I have brought with me also the *débris* of two calculi removed from two girls, aged respectively 3 and 7 years. The operation in the younger girl lasted forty-five minutes, and the stone, which weighed 183 grains, and consisted principally of phosphates, was crushed by the No. 6 lithotrite, and the *débris* drawn off by a No. 8 canula. The girl was perfectly well on the fourth day succeeding the operation. In the case of the elder girl the stone, a phosphatic one, was also crushed by the No. 6 lithotrite, but canulæ 6, 7, 8, 10, 12, and 14 were each employed once. She had suffered from dribbling of urine before admission into hospital. The operation lasted fifty-seven minutes, and her father took her away from hospital eight hours after it was finished. She lived not far from the hospital, and one of my assistants found her playing outside her hut four days after the operation, and, on enquiry, learned that

she was free from all urinary trouble, and had regained perfect control over the bladder. I do not think there can be a doubt that litholapaxy is the best and safest method of extracting the vast majority of calculi one meets with in the bladder of females of all ages.

In addition to the calculi removed from boys and girls, there are a few large ones on the table, which I have selected from my collection of stones removed from the adult male bladder. But time presses, and I would merely direct your attention to one which I hold in my hand. It is a fine specimen of the mulberry variety, weighing 956 grains, removed from a man aged 25 years. The operation lasted nearly two hours, and I crushed it principally by means of a No. 15 fully fenestrated lithotrite with a Bigelow's handle, made by Weiss and Son. It was a very tough one, as you can perceive, and I blistered the palm of my hand in crushing it. The man left the hospital quite well on the twelfth day after admission.

In conclusion, I would recommend the following rules to be observed in performing litholapaxy in male children and boys:— Use fully fenestrated lithotrites suited to the calibre of the urethra. Let all the canulæ be fitted with serviceable stylets. Never withdraw a canula from the bladder, or introduce one, unless it be fitted with a stylet. Let 4 or 5 ounces of water be the maximum quantity in the bladder at any given moment. Use the aspirator gently and methodically, and never inject water into the bladder while the patient strains. Be extremely gentle and careful in practising all manipulatious in the bladder and urethra. Be not in a hurry to finish the operation and, if possible, do not leave a grain of *débris* behind in the bladder. If all these conditions be fulfilled a large measure of success will be obtained.

Dr. Lewis Marshall acknowledged his indebtedness to the Society for having given him the opportunity to listen to Mr. Keegan's admirable paper. He gave his own statistics and expressed the opinion that an increasing experience made him confident that litholapaxy would be accepted by all surgeons as the operation for the majority of stones in children. This opinion had been arrived at in spite of his reluctance to set aside the time-honoured lateral lithotomy. In all cases requiring a "cutting" operation he would still adopt this form of lithotomy, because his own cases and those of past generations upheld it. The asserted ill-effects of this form of lithotomy were referred to by Dr. Marshall, who said that he could find nothing beyond the frequent assertion that emasculation was a common occurrence after a lateral operation in support of this defect, which

was anatomically probable. Two parents of children who Dr. Marshall had "cut" for stone had themselves had calculi removed by Cheselden's operation. For these reasons he still doubted that it could be accepted as an important argument against this method. He asked, "What evidence had we before us of the superiority of the sectio alba?" Perhaps a dozen reported cases, nearly all of which were exceptionally good results, and in all the stones might very well have been treated by litholapaxy. This method was fashionable, but it would not, in his opinion, be likely to be used, if the "crushing" and "cutting" operations were fairly weighed, for other than large stones. Dr. Marshall's cases bore out what Mr. Keegan expressed so ably in his paper, and he hoped that litholapaxy would now be more favourably received. In conclusion, Dr. Marshall asked to be allowed as a representative of provincial surgery to express his hearty thanks to Mr. Edmund Owen for his honest report of his unfortunate case.

Mr. John H. Morgan observed that the Society was to be congratulated upon the two communications which they had listened to that evening. First they had heard the straightforward statement of Mr. Owen, which would act as a caution against a similar disaster. Secondly, they had learnt from Surgeon-Major Keegan's own lips the details of his magnificent series of cases. With much of this surgeons who were interested in this subject had been familiar from papers in the 'Lancet' and 'Indian Medical Gazette,' but they had been favoured that evening with the exceedingly valuable and practical rules which Surgeon-Major Keegan had tabulated, and which would be of the utmost service. He (Mr. Morgan) had so recently given his views and experience in a paper read before the Medico-Chirurgical Society that he would not trouble the Society by repeating them. He had in that paper advocated the more extensive adoption of litholapaxy in the case of children, and had himself practised it in six cases, all of which had been most satisfactory; although encouraged to attempt it still more frequently by what they had heard that evening, there must, it seemed to him, be certain circumstances in which the operation was not practicable. The stone might be too large, or too hard, or there might be difficulty in grasping it, and lastly there might be difficulty in passing the lithotrite along the urethra. This had occurred to him on two occasions, but Messrs. Weiss had now so altered the instrument that it was not likely to happen again. The subject before them that evening was litholapaxy, but the previous speaker (Dr. Marshall) had launched off into the controversial field of lithotomy, but he would remind him that ample evidence was to be found of the existence of very serious after results of the lateral operation. He could recall from his own experience some six or seven cases of fistula between the bladder and rectum, of urinary fistula, and of incurable incontinence, whilst there was recorded proof of emasculation in a certain number of cases, and presumption of it in others. As regard the supra-pubic operation, he would be far from speaking lightly as to the ease of its performance or its freedom from risk. What then, he would ask, are the arguments against lithotrity? There were no immediate risks, as such an accident as had been related that evening could not occur if it were remembered that only the slightest amount of force was needed to drive a small quantity of water from the evacuator into the bladder. This could be seen by anyone who chose to experiment with a few pieces of coal in the bladder of a sheep or pig. It would then be seen that a very slight current set the fragments in motion, and that when pressure was relaxed the frag-

ments fell into the eye of the cannula and were drawn up in the regurgitant current. On the other hand, the operation was easy, effectual, and complete, though he agreed with Surgeon-Major Keegan that it was a more delicate manœuvre in the case of children and should only be performed by practised hands. As regards the theory that remaining fragments might form the nucleus of a fresh calculus, he agreed that this was improbable in the case of children, and it was always possible to make assurance doubly sure by using the evacuator on a second occasion. One reason against the wider adoption of this proposal was that cases of stone in children were not of frequent occurrence. Even at the Hospital for Sick Children the average was only about $4\frac{1}{2}$ per annum, and when these were divided between two surgeons it gave very small opportunity of carrying theory into practice.

Mr. Buckston Browne said that all honour was due to Mr. Owen for publishing his case. He was surprised that the vesical rent was extra-peritoneal, he should have anticipated an intra-peritoneal rupture. The rent being where it was, might it not have been treated as a supra-pubic cystotomy wound, and so the opening of the peritoneal cavity as well as the perineal operation have been avoided? In the consideration of the causes of such an accident there was something concerned quite apart from the patient, the operator, the stone, and the instruments, and that was the anæsthesia. The introduction of such an operation as modern lithotrity was entirely due to the use of anæsthetics, but it must be remembered that for safe washing out of the bladder the anæsthesia must be pushed to the period of complete muscular relaxation. If the anæsthesia was short of this, washing out the bladder with tube and aspirator was exceedingly dangerous, for on no account must there be any straining on the part of the patient, and the anæsthetist and the operator must be perfectly in accord. The preliminary stage was useful for sounding the bladder, when all noise was misleading and all stertor undesirable, but when the time came for washing out the bladder the anæsthetist must have the patient perfectly relaxed. Mr. Buckston Browne was very glad that Surgeon-Major Keegan had spoken of Mr. Clover with respect, for Mr. Clover was the father of the modern lithotrity aspirator. Mr. Clover's aim had always been to get the trap for the fragments of stone as near the end of the urethra as possible, and this led Mr. Browne to make the only criticism of Surgeon Keegan's paper which he would venture upon, and that was that there was not one of the tubes shown to-night which was not sufficiently long for a man, *i.e.*, about 8 inches long. In operating upon children there was no need for such long tubes, 4 or 5 inches being a sufficient length. The shorter the tube and the nearer the trap was to the end of the urethra, the more stone would be evacuated in a given time. In lithotrity in children, it was even more important than in adults to take care that before removal from the bladder both tubes and lithotrites should be perfectly free from every particle of stone, so that no risk should be run of leaving fragments of calculus in the urethra, rendering the re-introduction of instruments difficult and dangerous. He congratulated Surgeon-Major Keegan upon having thoroughly established the operation of lithotrity in children.

Surgeon-Major Keegan, in reply, said that stone in the bladder was common in certain localities in Central India, and that a number of branch dispensaries scattered over Central India fed the hospital at Indore, of which he held charge. The extension of railways in Central India enabled patients to reach Indore from outlying districts in compara-

tive comfort as contrasted with the long road-journeyings of former days, and so stone patients had flocked to his hospital in large numbers during recent years. The distribution of calculus in India was curious. The number of calculus cases in the whole of the Madras Presidency would not amount to more than, perhaps, thirty cases in the year, while at Hyderabad, in Scinde, a surgeon might treat as many as 200 cases in the same period. In Lower Bengal stone was not common; but it was very prevalent in the North West Provinces, in the Punjaub, and in certain localities along the Bombay Coast. He was not prepared to state what was the cause of stone, though no doubt food, geological formation, clothing, and meteorological conditions all played a part in its production. Mr. Owen had asked him if he had ever met with a medium-sized stone in a child which he had been unable to crush. He had met with such a stone on one or two occasions, and then he had at once performed lateral lithotomy. He was of opinion that Mr. Owen deserved great credit for the courage and surgical honesty displayed in relating his fatal case, and he then read the following extract from a paper written by him in the August number of the 'Indian Medical Gazette' for 1888, referring to a case of litholapaxy which had proved fatal as the result of kidney disease:—"I, however, feel pretty certain in my own mind that the fatal result in this case was caused by disease of the kidneys. But be the cause of death what it may, I have here to chronicle a fatal result following litholapaxy performed on a boy, aged 3 years, and as such I give the case every prominence. Litholapaxy as applied to the treatment of stone in male children is still on its trial, and must stand or fall by results. I have strongly urged its claims on the attention of my professional brethren both in this country and at home, and the more I practise this operation amongst boys the more I am satisfied with it. But I have no intention of bolstering up its claims to our acceptance by minimising or slurring over unsuccessful results which may follow its adoption. I hope that the advocates of the now fashionable supra-pubic lithotomy, more especially when they perform that operation for the removal of very small stones from the bladders of boys and young men, will also have the honesty to put on record their fatal cases. I am in a position to state that hitherto in England they have not always done so."

March 23rd, 1891.

ON PEROXIDE OF HYDROGEN, OR OZONE WATER, AS A REMEDY IN DISEASE.

(*Continued from a Research commenced in the year* 1858, *and reported to the Medical Society of London in the Sessions of* 1860–62.)

By B. W. Richardson, M.D., F.R.S.

Introductory and Historical.

In the year 1858 I was engaged in the study of peroxide of hydrogen with the view of introducing it into medicine as an addition to our treasury of remedies. The history of this singular chemical body, discovered in the year 1818 by the illustrious French chemist, Baron Thénard, had in 1858 almost become forgotten, and no one, as far as I could learn, had ever taken it up in respect to medical research; although, as I afterwards ascertained, a seat of learning at Haarlem did for some years offer a prize for a medical research upon it, which prize was withdrawn not very long before my work was entered upon, simply because there had never been a competitor. The peroxide when I commenced my investigations was so rare a chemical curiosity I could not obtain it in quantity, and was therefore obliged to set up the necessary apparatus for its manufacture in my own laboratory. In the manufacture I followed in every particular the instructions left by Thénard, and continued the manufacture until I could assign good reasons for advancing the fluid into the position of a valuable remedy in the treatment of disease. The great novelty of the peroxide solution—for the substance must be considered a solution, although it can be made to take the gaseous form—rendered the study of its physical properties alone of extreme interest, and, as it still remains a novelty to many, I may with advantage devote a few moments to a description of one or two of the special qualities of the solution; otherwise I might be talking an unknown language to some who are not conversant with certain primary facts, themselves unique in the history of chemical substances. This solution, then, is water containing, according to strength, so many atmospheres of oxygen. It is an oxygen atmosphere in solution. It is not, however,

a mere mixture, but a peculiar chemical compound. The oxygen can be made to accumulate, volume by volume, until the volume of water, say as much as would fill a pint measure, can rise to ten, twenty, thirty, and some say even over a hundred, volumes of oxygen before complete saturation is reached and a volatile body is formed. In the specimens on the table before us, prepared by Mr. Robbins, we have solutions of three strengths—viz., of ten, twenty, and thirty volumes. We hold, therefore, in a specimen of the peroxide condensed oxygen chemically combined either with the hydrogen of the water, or with the oxygen of the water, or with the two elements acting as a radical. There is here not much difference at first sight from what is common in combinations where there is accumulation of one element on another; as, for example, in the case of carbon with one equivalent of oxygen as in carbonic oxide, and carbon with two equivalents of oxygen as in carbonic dioxide. But now comes a distinction. The combination of the added oxygen in hydrogen peroxide is stable in the presence of some substances, unstable and easily evolved in the presence of others. Some substances, inorganic or organic, when added to the solution are neutral to it; other substances, inorganic or organic, evolve the oxygen and are themselves unchanged; a third kind evolve the oxygen, and with that some of their own contained oxygen; a fourth kind absorb the oxygen into themselves.

I will illustrate this by experiment. Here are six flasks equally charged with a ten-volume solution of the peroxide. To flask *one* I add a portion of oxide of iron: the action is neutral. To flask *two* I add black oxide of platinum; there is brisk action, so that I can light a taper from the oxygen thrown off. To flask *three* I add solution of permanganate; there is a very brisk action, and, with the action, evolution of oxygen from the permanganate as well as from the solution. To flask *four* I add arsenious acid: there is absorption of the oxygen by the arsenious compound. These are inorganic bodies; let me compare them with organic. Into flask *five* I put albumen; the action is neutral. Into flask *six* I put fibrin; there is brisk evolution. Thénard, in a general way, had a wonderful knowledge of these distinctions, but he did not live to follow them out or apply them. They are all important to our medicinal labours, in pointing the way to success by the experimental instead of by the empirical mode of questioning nature.

These observations led me very early in my research to form an original view as to the constitution of the fluid. When I liberated the oxygen from the solution, by one or other of the means described above, I found that the oxygen was neutral—that is to say, it presented neither ozonic nor antozonic qualities, but was like the oxygen of the common atmosphere in its natural state. When, however, it existed in solution in the form of peroxide, it responded to ozone tests, rendering the ozone test-papers of deep, dark colour, as I now demonstrate. These facts led me to conceive that the extra oxygen exists as ozonised oxygen, ozone, in combination with water, so long as it is in solution; but that, as it comes off from the solution, it escapes as atmospheric oxygen. This theory has met with opposition, for which I have much respect, but I still hold by it; and I call special attention to it here because, from a medical point of view, it is of primary importance. As I shall show, we use the oxygen of the solution in two forms: sometimes as a solution, when it is ozonic; and sometimes as a gas liberated from the solution, when it is neutral. The difference is great; it is the difference between ozone and oxygen, and until the profession understands that difference the utmost confusion must prevail in regard to therapeutical action.

It may gratify a new generation of Fellows of the Medical Society of London if I briefly relate what led me to use this solution in the first instance. It was in this way: I was working at ozone, and had been studying, with the late Dr. Moffat, of Hawarden, the changes of disease in what he called "atmospheric ozone periods." I made peroxide, and found that the oxygen condensed in it coloured Moffat's ozone test-papers of starch and potassium iodide, a fact which showed that this oxygen was more active than the oxygen of common air. It then occurred to me that the peroxide might be of service as a medicine if its action on the animal body and on the tissues were well investigated and defined. In those days we were inclined to believe that diabetes was due to the deficient oxidation of organic material—a theory which an original observation of mine, namely, that dogs could be made to eliminate urine containing glucose by subjecting them to an atmosphere of carbonic oxide, seemed to confirm.

As a result of this line of thought I made an original investigation, the results of which I reported at length in a paper read to the Medical Society in the session of 1860, describing the chemical,

physiological, and therapeutic action of the peroxide. I also fixed in this paper the best form of the peroxide of hydrogen for medical use, suggesting ten-volume strength as the most certain and convenient for ordinary use. At the meeting of the Society on March 3rd, 1862, under the presidency of the late Mr. William Coulson, I made a second report on the medicinal use of the peroxide. I had by this time used it in 223 cases of disease, including cases of phthisis, diabetes, anæmia, subacute and chronic rheumatism, strumous enlargement of the cervical glands, mesenteric disease, pertussis, chronic bronchitis, chronic laryngitis, mitral disease, and some forms of dyspepsia. On referring back to these two communications to the Society, I find in them a good deal of new physiological inquiry. I find that I had tested all the free tissues of the body to learn which of them did and which of them did not eliminate the oxygen by contact. I found that fibrin of blood and cellular tissue liberated the oxygen, that albumen and gelatine did not liberate it, that blood entirely deprived of fibrin absorbed the oxygen; and that common water deprived of all its gases so efficiently that fish could not live in it could be re-oxygenated and revitalised by addition of the peroxide solution, but not to the same perfection as obtains in water that has not been deprived of its natural air. It was also in these researches that I observed for the first time the remarkable influence of temperature on the action of the oxygen on the body; that heat caused oxygen to lose its power of sustaining muscular contractility, and that by it oxygen could be made a relaxant even of tetanic spasm.

What, however, will interest most the members of the Society will be a *résumé* of the therapeutical properties of the peroxide from those earliest papers. The *résumé* from the paper of 1860 was as follows:—(*a*) The peroxide quickens oxidation, increases secretion, arterialises blood, and hastens the decomposition of decomposing animal tissues. (*b*) In combination with blood it restores the power of muscles just dead to contract, and yet calms muscular irritability. (*c*) It promises valuable aid in a large number of cases of disease. (*d*) In instances of poisoning by narcotics and by the alkaloids it might with advantage be introduced into the blood by transfusion or injection. (*e*) In tetanus it affords a most rational treatment, since tetanic rigidity is relaxed by its presence in muscle. (*f*) In typhus, in which death occurs from what I have designated *asphyxia commencing in the blood*, it

might be employed with good effect as an oxygenator. (*g*) It ought to prove useful both as an internal and external remedy in the treatment of cancer. In the epitome of results in 1862 I drew as conclusions: (*a*) That in diabetes the peroxide reduces the specific gravity of the urine, whilst it rather increases quantity. (*b*) That in chronic and subacute rheumatism it affords relief. (*c*) That in valvular disease of the heart with pulmonary congestion it gives relief to the dyspnœa. (*d*) That in mesenteric disease and in jaundice it causes an improvement in the digestion. (*e*) That in pertussis its effect for good is very remarkable, since it cuts short the paroxysms of cough, and seems decidedly to shorten the period of the disease. (*f*) That in chronic bronchitis it lessens the dyspnœa, and renders the expectorated matter less tenacious. (*g*) That in chronic laryngitis it gives pain on being swallowed, and does not appear to be useful. (*h*) That in anæmia it does not of itself render a service, but favours the good effect of iron. (*i*) That in the first stage of phthisis it causes improvement in the digestion; and in the later stages gives unquestionable and even wonderful relief to breathlessness and oppression, acting, in fact, like an opiate without producing narcotism, and assisting oxidation.

But the most important new observation I had to communicate to the Society in 1862 was that in free and frequently repeated doses the peroxide could be made to produce a modified salivation, a fact which led to two suggestions; one, that in the use of the mercurial and iodide preparations it might be the chlorine or iodine in them which caused the ptyalism; secondly, that the peroxide would be a good substitute for mercury and the iodides in the treatment of syphilis. In the discussion which followed upon the reading of this paper I was warmly supported on several points by Drs. Septimus Gibbon, Symes Thompson, and Gibb, all of whom had been prescribing the peroxide on the suggestions made in the previous paper of 1860. Dr. Gibb also bore special testimony to its value in affording relief during the last stage of phthisis, for which I had prescribed it in the case of a member of his own family.

Such in the briefest terms is the history of the introduction of the peroxide of hydrogen, as it is commonly called, or ozone water, as I should call it, into medicine. Soon after it was thus introduced it created a great deal of discussion, but owing to difficulties

in its manufacture and some instability of the earlier specimens of it, it fell out of favour, except in two unexpected directions. An incidental observation of mine that the solution gave a golden colour to feathers led to the employment of it as a golden hair-dye; while a similar reference to its effective action as a deodorant and disinfectant led, I believe, to its use in that very excellent compound called "Sanitas." Occasionally also it was employed in medicine, and now and then I was consulted in regard to its employment. Matters remained in this state until about five years ago, when by some sudden and inexplicable impulse the peroxide began again to be popular in medical practice, especially in America, and from that time onwards I have had communication with practitioners, both at home and abroad, who were anxious to be informed on points of a practical kind relating to it. It was this circumstance that led me three years ago to propose to revive the subject in the Medical Society of London, and finally to bring forward the present paper. This was the more necessary because, although practitioners who are now largely prescribing the remedy have often, in the most handsome manner, assigned to me the credit of originating the practice, they do not seem to have seized on the principles which led me to introduce it, nor to see the compound nature of the fluid and the different lines of practice which spring from that recognition. Meantime, in the twenty-nine years that have elapsed I have continued to work at this research; practically in the steadiest manner, theoretically by fits and starts, as the opportunities of my overcrowded life have permitted. Now, as a kind of continued report, I propose to give a further account of my stewardship during the gap of time that has supervened since I last spoke on this topic from this place.

New Methods of Application.

In carrying out this idea let me notice in the first place a few new facts bearing on modes of administration which did not in any way belong to my previous communications to the Society. Some time about 1863 I made an observation regarding the behaviour of the peroxide with ether. In agitating a thirty-volume solution of the peroxide with anhydrous ether I found that the ether became ozonised, and that it was in this way possible to make a new and useful medicinal preparation. To the preparation so formed I gave

the name of ozonic ether, and from the time when it was first announced to the present it has been utilised. The late Dr. Day, of Geelong, in Australia, sent to me for the mode of preparation, and, having learned to use the ether in his own practice, devoted to it much study up to the end of his valuable life. He warmly supported my application of the fluid in the treatment of whooping-cough and its use as an inunction after scarlet fever, and it was he who first employed this compound with guaiacum as a blood test. The admixture of the peroxide with a volatile compound like ether led me to another modification—namely, the administration, by inhalation, of the oxygen evolved either as oxygen, ethereal oxygen, or ozone, for I soon learned that it could be administered, as I shall show presently, in all these states. This was a distinct advantage, as it enabled me to move from the mode of administration by the mouth to a larger and, in cases of lung disease, a more direct method. Taking further advantage of the ozonic properties of the peroxide in solutions, I introduced it in 1865 at the meeting of the British Medical Association at Chester for the deodorisation and disinfection of rooms in which patients suffering from infectious diseases are under treatment, and also for application as spray to fetid or ulcerated surfaces, as in diphtheria, syphilitic ulceration of the throat, and gangrene, for which purpose it may be used either as ozonic ether or simply as the aqueous solution. In addition to the above new progressions I have made others of a physiological and practical kind, bearing on other possible methods of administration. Some of the facts disclosed in this direction are published in the last 'Asclepiad,' and I may therefore be content by stating of them in summary, that it is practical to inject the peroxide solution subcutaneously; also, under some circumstances, into mucous and serous cavities, and even into the pulmonic structures. Lastly, I would report in this place that I have tested the effects of morbid substances, especially purulent matter, on the peroxide, as I had previously tested effects of normal structures.

Old Experiences in Practice.

Let me now proceed to compare the practical experiences I have gained in the use of the peroxide since 1862. In the previous reports I dealt with many cases of disease in which I had applied this remedy; and in respect to all the forms of diseases specified

I may say that in the long experience which has succeeded I have nothing of moment to retract, little to modify, much to emphasise and support.

Diabetes.—I have many times prescribed the peroxide solution in this disease since 1862. In four cases, specially noticeable because occurring in members of our own profession, the patients were all satisfied of benefit from it, one of them taking it during a period of eleven years. The results have always been that it reduced the quantity of sugar, but did not reduce the quantity of water eliminated, unless it were combined with codeine, with the use of which Dr. Pavy has made us so familiar. Codeine *three* grains, alcohol (sp. gr. 0·830) *two* fluid ounces, solution of peroxide of hydrogen (ten-volume strength) *two* fluid ounces, distilled water to make *twelve* ounces, to form a mixture of which half a fluid ounce may be taken three times a day in a wineglassful of water. This is by far the most useful prescription I have ever used in diabetes.

Phthisis Pulmonalis.—In the treatment of phthisis pulmonalis the solution has been constantly in use, and in it I have found the greatest service. In some cases of this disease, in which the solution has been borne in large doses during the early stages, it has led to such remarkable results towards recovery that I might almost have been pardoned if in younger and enthusiastic days I had declared in it the discovery of a specific. Happily a natural fear of the *experientia fallax* held me back from such a course; but the fact ought not to prevent me from stating that in the first stages of phthisis it is of all medicinal remedies I know the most valuable, and that in the later stages, when dyspnœa is the distressing symptom, it is, especially in combination with ozonic ether, the remedy that gives most relief. It soothes, and it seems to supply oxygen to the blood, and so act vicariously, to a small extent, for the purpose of respiration.

Pertussis.—I repeat all I previously said about the value of the peroxide in the treatment of whooping-cough. I am sure there is nothing so near to a specific for this affection. The only difference I have instituted in the employment of it in this disease consists in prescribing it in the form of ozonic ether. I prescribe it for children in this form, in doses of from 10 to 60 minims, diluted with half a fluid drachm of spiritus tenuoris and 4 drachms of water, to be taken four times a day, with a little more water if

that be preferred. But I also prescribe the ozonic ether in this disease by inhalation from the Siegle's steam spray inhaler. In one instance, where the paroxysms were as severe as I have ever seen them, they were arrested at once by this inhalation, and were held in such control by continued inhalations that what may be called a "a cure" was the immediate result. As to the power of the oxygen to control muscular spasm, I repeat with still more insistance its value. In practice I have availed myself of the peroxide with ether, not only in whooping-cough, but also in asthma and in angina pectoris. Here the ethereal solution can be used by inhalation in its simple form, or combined with amyl nitrite, or other body of the antispasmodic class that will admix with ether and diffuse with it. In these affections I administer by inhalation in a manner that will be demonstrated in the last section of this essay.

Local Use for Ulceration and Purulent Exudation.—In the paper of 1862, I recommended the use of the peroxide for external application in cases of surface decomposition, ulceration, and purulent exudation. I have to emphasise this use. I will name one case alone in illustration. I was called to see a gentleman advanced in life who was suffering from moist senile gangrene of the foot. The extremity was so offensive and the separation so slow, although there was a fair line of separation, that the management was most distressing. I had the foot wrapped in cotton wool holding peroxide solution slightly acidulated with dilute hydrochloric acid. The offensive odour was at once removed, and the process of separation went on rapidly; the foot soon fell off, leaving an excellent healing surface. The patient lived many years afterwards in fair health, and died at last of mere old age.

Syphilis.—I restate the value of the peroxide in syphilis, and especially in all the chronic stages and conditions of syphilis in which mercury or potassium iodide would, usually, be resorted to. It seems to me, in these cases, to have an intermediate action between mercury and potassium iodide. It is also useful in indolent syphilitic eruptions, and it has the great advantage over both mercury and the iodide that it leaves no after-effects of an injurious character. Salivation from large and frequently repeated doses is soon recovered from when the fluid is withdrawn, as if the ptyalism had been induced by a volatile substance. It also aids the action of mercury, rendering much smaller doses of that remedy effective.

The property exemplified in this action is that of elimination by excitement of glandular function, an excitement probably always sustained by the oxygen of the tissues in normal states of health, and only accelerated by the active oxygen. I have of late years become so confident of the value of the peroxide in the treatment of syphilis under the conditions in which mercury or iodide come ordinarily into use that I have employed it as a matter of course. I use it in the primary as well as the secondary form of the disease, and for those cachectic lesions in the syphilitic constitution to which the term "tertiary" is applied there is, I think, no remedy equal to it. In obstinate cases of ulcer and cutaneous eruption it goes admirably with the mercurial bath, and may be prescribed in doses of from *two* to *four* drachms three times a-day with full doses of fresh cinchona infusion. The solution also has its local use as an excellent injection into the nasal cavity when there is syphilitic ulceration of cartilage and of the spongy bones.

New Experiences.

From these references to practice based on the old observations I pass to some observations of a practical nature which have not before been recorded. I might have dealt longer on old experiences in respect to mesenteric disease, epilepsy, and anæmia; but, as my mind is not so satisfied in regard to any advance in these directions, I let what I have already said remain in its original form, in order to give room for what is new. I also allow to stand over for future comment some notes on the action of the peroxide on digestion and in dyspepsia.

Use in Diphtheria.—Owing to the observation made on the effect of the peroxide solution in causing separation of dead from living structures, I have several times used it since 1864 as a local application of diphtheria, in the treatment of which hitherto inscrutable disease I have had the most painful of human experiences. Applied to the throat in diphtheritic patients, the solution of peroxide, either alone or in combination with glycerine, causes, without any doubt, a rapid separation of the false membrane, and produces a favourable condition of the local surface. But here, I regret to say, the benefit—and it is a benefit—ends. On the general or constitutional conditions it has not as yet, according to present modes of using it, been efficient. It does not touch the collapse. I name this because I see that some writers, observing the good local

effect in cases that have, for some other reason, recovered, have, on the *post hoc et propter hoc* principle, proclaimed the peroxide a specific in diphtheria. It has not won that position. At the same time, I invariably prescribe it locally in diphtheria, not only to the throat, but as a wash for the nostrils when there is ichorous and offensive discharge. The twenty-volume solution can be applied freely to the fauces, with borax or with glycerine, and to adults it can be administered in the form of spray.

Use in Intestinal Cases.—The administration of the peroxide solution as an injection *per anum* was a line of practice that occurred to me about 1870 in a case of cancer of the rectum, accompanied with a free and very offensive discharge. The action of the solution in destroying the odour, and, combined with tannin, in limiting the discharge, was most useful. Soon after that I was summoned to a patient suffering from what I may designate as strumous caries of the sacrum, with adhesion and ulceration of the lower intestine, and copious escape of pus into the rectum. A more painful case, or one more difficult to manage, I do not remember to have seen. Here I prescribed the ten-volume solution, with tannin and dilute hydrochloric acid, by injection, with the best results. The disease proved fatal, for the whole of the sacrum was involved before the use of the oxygen solution was commenced, and after paralysis of the lower extremities had supervened. But the solution exerted such a controlling influence, and rendered the discharge so healthy, I could but regret that the treatment by it had not been brought into action at an earlier stage.

In a case of what was called chronic dysentery, with frequent motions of a fluid and offensive character, I prescribed the ten-volume solution, with tannin, in ounce doses diluted with a pint of tepid water. The injection was thrown into the bowel once daily through a long O'Byrne tube, and, as the oxygen was liberated freely in the intestine soon after the injection was thrown in, the evidence was supplied of the excretion of purulent fluid, or of some excreted matter from blood. The result was very satisfactory, and led to such a rapid and sound recovery that I can recommend the repetition of the practice with the utmost confidence.

Use by Inunction.—From a similar line of thought and practice I have for many years used the peroxide as an inunction for the surface of the body of patients affected with cutaneous exfoliation,

as in scarlet fever and measles. In these instances the ozonic ether is used instead of the aqueous solution, because of the facility with which it mixes with the oil or lard required to make an ointment. The body is rubbed from head to foot with the ointment, the spaces between the fingers, toes, and flexures of joints being included. The application is very agreeable to the patient, and the effect in removing obnoxious odour is most marked. But the grand advantage lies in the circumstance that the process seems to remove all danger of infection from the convalescent after three or four applications.

Use in Eczema.—As a general rule, oily and fatty dressings do not agree well in chronic eczema. But I once gained considerable advantage by treating a chronic eczema, which had previously and for a long time been most intractable, with an ointment of spermaceti saturated with ozonic ether. In other eczematous cases, with exudation of blood, I have employed the aqueous solution with tannin with great advantage. In cases of this nature, where there is much irritation, the solution goes well with an anodyne.

Uses in Diagnosis.—I have found the peroxide of hydrogen of great service in diagnosis. It diagnoses pus from mucus. In cases where I am in doubt whether matter expectorated by the lung is or is not purulent I perform the following test. I place the expectorated matter in a test-tube containing the solution. If the matter causes no evolution of oxygen, there is no purulent matter present. If the expectoration produces liberation of oxygen, the evidence of pus is strong; and if the same event occurs after all carbonic acid is removed, the presence of either pus or of some plastic element of blood or cellular tissue element may be affirmed of a certainty. In the first test-tube which I now take up there is secretion I do not suspect, and on adding to it some of the twenty-volume solution there is, as you will see, no action whatever. In the second tube there is some doubtful secretion; but again we see there is no action. The third tube holds a few minims of secretion strongly suspected to be purulent, and on addition of the solution the vehement action confirms the suspicion. In the large majority of instances it will be pus that is present, but the diagnosis extends to cellular tissue. Injected into a closed mucous cavity, like the bladder, the surface being in a healthy state, and the viscus having been emptied of urine and washed out with warm distilled water, there is no liberation of

oxygen from a neutral solution of peroxide. But if there be any ulceration, by which a surface of cellular tissue is laid bare, or if there be pus present, then the escape of oxygen from two or three ounces of a ten-volume solution used as injection will be so free that the gas may be detected as it escapes from the free end of the catheter by its effect in relighting a partly extinguished taper. The same test might be applied for the diagnosis of ulcerated mucous surface, or presence of pus in the uterine cavity, and possibly also the stomach, but no opportunity has been offered me of testing in these last-named cases. I may, however, add that no more mischief could follow from the test than would follow from the injection of common water or common atmospheric air, if the solution made be chemically pure; on the contrary, the ozonic quality of the oxygen in the solution would tend to have a good effect. It might be sound practice, indeed, to sustain in some forms of ulceration a gentle continuous current of the fluid through a mucous cavity. In extensive ulceration of the bladder or uterus, accompanied with offensive discharge, this treatment offers promising results.

Suggested New Practices from Past Experiences.

In the course of original research it often happens that a number of side suggestions and thoughts occur to the mind which were not dreamed of when the research commenced. In this way theory is brought out which, being an induction from experiment, may easily become practice, and that, too, of a valuable character. As a rule, it is good for an investigator not to advance theory, even of this order, until he himself has put it to the test. But there are exceptions to every rule, and as some suggestions, leading to important lines of practice which call for proofs I can hardly hope to make in my own lifetime, but which younger men may have the opportunity of making, have forced themselves on my attention, I proceed to state them without reserve ; and this the more readily because, really, in some instances the way towards practice has been opened for a considerable distance with perfect safety.

Let me take, as a first illustration of this kind, an extension of the practice of injecting the peroxide solution into mucous canals. I have indicated, as a practical fact, that the solution can be injected into the alimentary canal, and that, in more than one

example, it deodorised and neutralised an offensive intestinal discharge in a most effective manner.

Typhoid.—I would venture further. I would recommend the practice in the treatment of typhoid fever. In typhoid fever it would be possible to throw into the canal by this means twenty pints of ozonised oxygen per day by using one pint only of the ten-volume solution. The gas, when liberated, would diffuse through the whole of the canal, and would decompose and oxidise those decomposing exuded products, which, by secondary absorption, are the cause of the relapsing febrile seizures, with an efficiency possessed by no other remedy. It would be like exposing the decomposing ulcerated surface to ozonised sea air.

Hypodermic and Subpulmonary Injection.—As in physiological research I found no danger from injecting the neutral solution of the peroxide into the cellular tissue, I would venture the same on the human subject, and I fully expect that the practice will in time be largely adopted. In asphyxia, as after drowning and after suspended animation from chloroform and other narcotics, 100 pints of oxygen might easily be transfused, hypodermically, by using the thirty-volume solution; and, with the temperature of the solution raised to blood heat, the diffusion of the gas through the cellular tissue would oxidise the blood rapidly, and tend to give a new force to the circulation. I should even go one step further in extremest cases. In sudden collapse from chloroform, if the natural respiration had actually ceased, and death seemed to have occurred, I should, to save time, whilst sustaining artificial respiration, inject the lung cavity directly on both sides of the chest with the solution. The diffusion of the gas through the lung structure would arterialise the blood remaining in the lung, while the mechanical pressure exerted within the pleura against the elastic structure would be a motor to the circulation, compressing the blood towards the left side of the heart, and producing a current of blood from the right side. For these subcutaneous and subpulmonary injections of the peroxide, I have invented a new automatic syringe, which can be used without withdrawing the needle, the piston movement being sufficient for filling as well as for emptying the syringe. There would be no danger of injecting four ounces of the thirty-volume solution, rendered neutral and warmed to the normal blood temperature, into each thoracic cavity by needle puncture through an inter-

costal space high up in the fore part of the thorax. The water might produce a little œdema, but it soon would be picked up and carried into the circulation by the capillary vessels. Bearing also on this subject of subpulmonary injection, there is another use of the solution which has often occurred to my mind, and which would be perfectly legitimate as a practice, when the powerful action of the peroxide in destroying decomposing and purulent matter is taken into account. I suggest that in cases of cavus of the lung in phthisis, where the cavity can be clearly defined, there would be no risk of injecting the cavity, through a fine needle, with the solution, and of oxidising all the fluid that it might contain. Again, in dangerous hæmoptysis it would be most legitimate practice to inject into the pulmonary structure the oxygen solution saturated with tannin. The oxygen could do no harm, and the tannin, which would be quickly transformed into soluble glucose, would merely charge the blood with a useful saccharine food, with the advantage of making the lung, in a small degree, a kind of second stomach. Still more, it is quite worthy of serious consideration whether it would not be good practice to inject the solution in small quantities by the subpulmonary method in the early stages of phthisis. The oxygen would destroy tuberculous exudation more quickly than any other agent, and there can be little danger in diffusing oxygen through an organ made for the sole purpose of receiving and condensing oxygen. The one danger would be hæmorrhage, and that could be met by combining tannin with the solution. I name this new mode of treating the lung substance "the subpulmonary." In a word, my contention is that we may learn to transfuse the peroxide solution without risk and with benefit into the lung, and indeed into any organ of the body, not excluding the brain and nervous centres.

The One Danger.

The one danger that could follow would arise from delivering into the tissues so much oxygen at once that the blood would not absorb it with sufficient rapidity to allow afterwards a free course through the minute circulation. I will show an experiment on this point in illustration. Here is a two-necked flask charged with six ounces of defibrinated blood from an ox. The blood has been deprived of all free carbonic acid by being washed with oxygen. It is of bright arterial colour, and if a current of air, free of

carbonic acid, be driven through it into lime-water, there is no trace of carbonic acid. But when to the blood I add neutral peroxide solution, there is, even though it is already arterialised to some extent, brisk action. There is more oxidation, there is a slight elevation of temperature, and there is copious escape of gaseous matter through the tube and through the lime-water in the flask. The gaseous matter escaping consists partly of liberated oxygen, but, as shown by the precipitate in the lime-water, there is also carbonic acid; and, had the blood been venous the amount of carbonic acid would have been very considerable. Thus it is clear that liberation of carbonic acid in the venous circuit may occur from the use of peroxide; and, as there is no escape within the circuit for the gas or gases, there is the same danger as there would be from blowing air into a vein. I observed this fact in an experiment recorded in the last number of the 'Asclepiad,' in which I restarted respiration in a lower animal, after death from chloroform, by injecting peroxide solution into the peritoneal cavity. The respiration was so good that life seemed to be returning until the vital functions were arrested from some other cause telling upon the circulation. After death it was found that the right side of the heart and large veins were filled with a bright arterialised blood, richly charged with oxygen gas that had been absorbed from the large veins, and with which carbonic acid gas was also admixed. The prevention of this source of danger has to be worked out. It may be done by using a weak peroxide solution, or by using it as an alkaline solution. Meantime, the danger ought to be known, and the nature of it understood, since its existence, without being understood, might interfere materially with the proper application of the peroxide in its most useful and innocent administrations.

Use in Tetanus and Hydrophobia.—I have spoken of the effect of the peroxide in relaxing muscular contraction, and the facts I have observed in this direction lead me to suggest its use in two particular diseases of the spasmodic type,—hitherto most intractable—I mean tetanus and hydrophobia. Anyone who has watched the course of one at least of these diseases (tetanus) will feel that the remedy the most of all hopeful, if it could be realised, is a means for preventing the muscular tetany, for producing free glandular elimination, and for causing free oxidation of the septic blood. In the peroxide solution we have all these requisites, and in the next

case of tetanus that comes under my care it is my intention to put the remedy to the completest trial. I shall inject the muscles in groups with the neutral ten-volume solution, heated to blood temperature, adding, if the oxygen be not alone efficient in reducing the tetanus, a weak alcoholic solution of amyl nitrite.

Mechanical Use.—One other purpose for the peroxide must be named—a purpose purely mechanical, and yet not to be slighted. In the process of being liberated from its solution, the gas exerts an expansion which, in many instances, may prove of great service. In this mechanical mode of application it could be used in obstruction of the bowels from intestinal involution. It could be used for causing distension of the bladder when the sound is being employed, and perhaps in stricture for producing dilatation of the passage posteriorly to the stricture. In the same manner it could be brought into play for dilating the uterine cavity; while for bringing on premature labour, in instances of contracted pelvis, the effect of the equal distension by the gas liberated from three or four ounces of the ten-volume solution would, I suspect, be as rapid as it would be safe and effective.

New Pharmacological Research.

Let me describe, in the last part of this essay, some pharmacological outlines. Peroxide of hydrogen is still made, as Thénard first made it, by the action of an aqueous acid solution on peroxide of barium, the number of volumes in each specimen turning on the number of precipitations. I originally proposed a ten-volume solution for medical purposes, and that has been well adhered to; but for special purposes a solution of twenty and even thirty volumes may be employed. In order to keep the oxygen in the solution, it is necessary to have it slightly acidified, either by dilute nitric, hydrochloric, or phosphoric acid; and, for my part, I prefer the dilute phosphoric acid. In this form the solution can be administered internally in doses varying from a fluid drachm to a fluid ounce; but two or four drachms, diluted in ten fluid ounces of water, may be considered an average dose. The taste is peculiar, being identical with that of ozone when inhaled by the mouth, and not at all dissimilar to the taste of chlorine in a weak solution. For injection purposes, the solution should be rendered neutral by the cautious addition to it of caustic soda or potassa. The soda, I

think, is the best; but it should not be neutralised until the time when it is wanted. In ten volumes it may be injected either into the cavities or into the cellular tissue without being further diluted. The solution of the peroxide is incompatible with many substances; but it is compatible with the following: the mineral acids, tannin, alcohol, glycerine, alcoholic solutions of the alkaloids, and alcohol combined either with ether, amyl hydrate, amyl nitrite, or isobutyl.

Administration by Inhalation.

The oxygen from the peroxide can also be administered by inhalation by means of a simple apparatus which I have constructed for general use, and in working which I have taken advantage of another oxygenated body that liberates the oxygen and at the same time yields up a portion of its own oxygen. Into a glass vessel having two necks, like the ordinary Woulffe's bottle, a few ounces of the peroxide solution are placed. A tube from a funnel, the tube having a stopcock, runs through the stopper of the bottle to the bottom of the contained solution. From the second neck of the bottle there springs another tube for inhalation, at the free end of which is a double-valved mouthpiece, like the oldest and best chloroform inhalers. The apparatus is now ready for use. When it is wanted the funnel is charged with a solution of permanganate of potassa in the proportion of *ten* grains of the permanganate to *one* fluid ounce of water. The tap is then turned, the permanganate solution flows into the peroxide solution, and, immediately, oxygen derived from both bodies is briskly evolved. The gas, if necessary, can be caught in a flexible balloon, or bag, from which it can be inhaled in measured doses, but this is not a requisite. Should the practitioner desire to combine the oxygen with ether, or with any other volatile body, soluble in ether but not in water, he can allow the ether or ethereal solution to float on the top of the peroxide solution. Then, as the oxygen escapes, the ether, or the ether with any other volatile body, passes over in combination with the oxygen. A patient can help himself to a dose at any time, as may be directed, by merely adding a measured quantity of the permanganate solution. Nitrite of amyl can be inhaled in this manner with ether and oxygen, and there is here a means of administering most of the active volatile medicaments by inhalation, and

in a form ready for combination with the blood, as it passes in its course over the pulmonic circuit. How easily the apparatus can be brought into use will be seen by the demonstration now made.

Under the form of ozonic ether, the administration can be carried out either as a medicine to be taken in water, or as a spirituous solution by the mouth, in doses of *half* a fluid drachm to *two* fluid drachms, or as fine spray from the Siegle steam inhaler. For a styptic and deodorant spray for the throat the following form is useful: alcohol, pure (sp. gr. 830), *half a fluid ounce;* tannin, pure, *twenty* grains. Dissolve the tannin in the alcohol with *one* ounce of distilled water, and then add solution of peroxide of hydrogen (ten-volume strength) to *six* fluid ounces. To make a solution for spray.—Of this solution, *one* or *two* fluid drachms can be used with Siegle's spray inhaler, a mode especially applicable in cases of throat affection. When the peroxide spray is brought into use for the throat, it is good practice, in some instances, to make the spray the means of administration, in small doses, of medicinal agents which may act internally. Quinine may be administered in this manner, and also soluble preparations of opium, and many other substances that are soluble in alcohol, and are, at the same time, compatible with the oxygen solution.

Spray for the Sick Room.

Reference was made in the first part of this paper to ozonic or sea spray for the sick room—a solution diffused in the air as spray from a spray producer. The solution is composed of the solution of peroxide of hydrogen (ten-volume strength), *ten* ounces; common salt, *two* drachms. If a pleasant and pine-tree odour be desired, a few drops of ethereal tincture of Kaurie gum can be added, and the effect can easily be produced. In sick rooms and in the wards of hospitals such sprays could be arranged to work automatically so as to deliver and diffuse the solution at regular intervals. For this purpose Messrs. Krohne and Sesemann have constructed for me an apparatus which will distribute spray for several hours at a time.

Other Modes of Application.

The peroxide solution can be prescribed as an antiseptic and

astringent gargle. A good form for this purpose is: tannin, pure, *ten* grains; glycerine, pure, *one* ounce; distilled water, *four* fluid ounces; dilute hydrochloric acid, *ten* minims; solution of peroxide of hydrogen, sufficient for *eight* fluid ounces. To make an *eight*-ounce gargle. An evaporating lotion can be made with the peroxide in the following form: alcohol, pure (sp. gr. 830), *two* fluid ounces; anhydrous ether, *one* fluid ounce; distilled water, *four* fluid ounces; solution of peroxide of hydrogen (of neutral reaction and ten-volume strength), to *twelve* fluid ounces. To make a *twelve*-fluid-ounce lotion.

Another simple yet useful preparation of the peroxide is as an unguent, for inunction over the surface of the skin after scarlet fever, and other affections where there is free shedding of dead cuticle. The unguent is made by rubbing up ozonic ether with spermaceti ointment, so as to form a thin paste, which may, as desired, be rendered thick by the addition of a little more spermaceti, or thin by addition of olive oil.

Administration by Injection: An Automatic Injection Syringe.

In order to inject the peroxide solution into the cavities or beneath the skin, I have invented a syringe which I call the "automatic syringe," and which Messrs. Krohne and Sesemann have very neatly constructed. It is, as will be seen, a small graduated instrument, having a feeding-tube for charging it on one side, and an exit-tube for discharging it on the other side. The syringe is automatic in the sense that when both tubes are in place, the mere act of working its piston up and down fills it and empties it. The quantities of fluid received and delivered by it vary from *ten* minims to *four* fluid drachms, and at the end of the exit tube it can be fitted with injecting needles of different sizes or with a nozzle, according to requirement. With this syringe, the peroxide solution can be injected through a needle subcutaneously, or into deeper structures. By attaching the delivery end to an enema tube it can be used also for the intestinal injection, and by attaching the same end to a catheter, it can be employed for injecting the bladder. The advantage in every case is that the current can be kept up when once it is started, either for a small or large injection, and that the admission of air is impossible. The syringe might be used without any complication for the trans-

fusion of blood and for aspiration, as well as for transfusion; but it has been specially adapted for the oxygen solution.

Conclusion.

In closing these pharmacological notes it is well to recall the important fact that the remedy we have had before us has about it the peculiarity that it is a purely natural remedy, that it is not toxic, and that with the exception of the danger of producing by it gaseous accumulation in the blood, under some circumstances of use—a danger which admits of being avoided—it is free of all risk. In its remedial application, too, there is no complexity as to its *modus operandi*. In every instance that which it effects is oxidation. For asphyxia it is oxidation; for spasm it is oxidation; for breaking up pus and exudations it is oxidation; for destroying infectious and fœtid organic secretions or excretions it is oxidation; for producing eliminative action it is oxidation.

At the close of the paper on peroxide of hydrogen, read before the Medical Society of London in 1860, I said:—"Sir, in placing this memoir on the annals of the Society I guard myself, once and for all, from any exaggerated suggestions as to the value of this new medicinal agent. The subject is so novel, even to me after many months' learning and knowledge of it, I have feared to use a sentence that has not been measured and recast over and over again. I am not offering this medicine as an *elixir vitæ*. I do not pretend to know all its properties. I do not bind myself irrevocably to that which, at the present meeting, has been offered; on the contrary, I remain open to all new observation and knowledge for correction and guidance. I believe we have here within our grasp a powerful medicine, the full value of which it may take years to develop, and the useful application of which may be developed in directions different from those now advanced. My purpose will be served if I call forth investigation and elicit fact, let the course of events bend as they may."

A generation has passed since these words, Sir, were spoken, and twenty-nine Presidents have filled the place you occupy to-night. But to you and my fellow-students I re-echo those words; and, although I have spoken with more knowledge, experience, and confidence in a remedy which forms the most active and preponderating element of the healthy living body, I wish still to express

the same earnest hopes with the same candour and the same respectful confidence in the voice of the future.

Dr. Routh said he had had the privilege of hearing both Dr. Richardson's previous papers. He had been much interested by the remarks made by the surgeons present. He could, however, refer to his knowledge of the drug as a physician. In three classes of cases he had tried it and found it most useful: (1.) In cases of phthisis, where it seemed in some way to act as cod-liver oil, and was well borne in cases where the oil was not tolerated. (2.) In cases threatening asphyxia, during pneumonia or bronchitis, where given in ℥i doses, it appeared to oxygenate the blood, strengthen the patient, and hasten recovery, especially in cases of great debility. (3.) In cases of mental weakness and premature decay, where persons had overworked themselves, so that study or special attention in reading or thinking was not possible owing to lassitude, exhaustion, and loss of memory, ℥i doses of this drug seemed to remedy these symptoms and recuperate the patient.

Mr. Spencer Watson mentioned, in illustration of the value of peroxide of hydrogen as a means of diagnosis, that Dr. Mooran Brown, of Chicago, had employed it for the injection of the antrum in cases in which suppuration within that cavity was suspected. The result had been that, if pus was present, the moment the peroxide reached the cavity the nostrils became filled with a whitish foam. The injection had been made through a canula passed through the normal aperture in the hiatus semilunaris. This method would be very difficult in some cases, and it might then be preferable to puncture the thin inner wall of the antrum by means of a curved trocar and canula. There could be no doubt of the value of the peroxide of hydrogen for the purposes of diagnosis in suspected suppuration of closed cavities, or in deep seated suppuration generally.

Dr. Richardson briefly replied.

April 6th, 1891.

CASE OF FIBRO-SARCOMA OR "DESMOID GROWTH" OF THE ABDOMINAL WALL.

By Alban Doran, F.R.C.S.

F. C. T——, aged 21, a healthy, corpulent young woman, of short stature, came under my care at the Samaritan Free Hospital in the summer of 1890. She complained of a tumour in the left groin, which gave her discomfort and alarm.

On July 6th I examined the patient. A very firm, solid tumour projected immediately above Poupart's ligament. It was oval, measuring $5\frac{1}{2}$ inches in its long diameter, by 3 inches vertically;

its long diameter being parallel to the crural arch. Its internal and blunter extremity lay close above the spine of the pubes, no part of the tumour passing to the right of the middle line. The outer and more pointed extremity came close to the anterior superior spine of the ilium. It was movable and not tender, save on firm pressure. The uterus was freely movable, and its cavity measured $2\frac{1}{2}$ inches. No connection could be traced between any part of the pelvis and the tumour.

The patient had been married four years, and had borne two children. She also miscarried in November, 1888. Her youngest child was four months old; she was suckling it, and the catamenia had not returned. Two years and a half before I examined her she first noticed the swelling. It was about the size of a pigeon's egg, and occupied the region where now lay the outer extremity of the tumour. She noticed that it gradually extended downwards and inwards as it grew bigger. This account of its seat of origin did not accord with its attachments as detected at the operation. Moreover, she stated that when it first appeared she went to a large hospital, and a dresser said, "It was a rupture," and prescribed a truss, which was accordingly applied. To take a tumour near the spine of the pubes for a hernia is an error both natural and probable. On the other hand, it is not very likely that even a student would diagnose a small growth near the iliac spine as a hernia. Patients, however, are often very inaccurate in localising origins of tumours; this is particularly the case in ovarian cystic disease, which is often described as first appearing in the groin, even when the surgeon himself has examined the tumour in its earlier stages, and noted that it first rose into the hypogastrium. The truss was worn for eighteen months, then a chemist examined the patient, and said that there was a tumour. She became pregnant, and was advised to go to the Samaritan Hospital after the birth of her child. The child had just been weaned when she was admitted under my care. The patient had no evidence of any visceral disease.

On July 12th, 1890, I operated. I divided the integuments in the long axis of the tumour; the incision, therefore, ran almost parallel to Poupart's ligament. The aponeurosis of the external oblique muscle was thus exposed. On dividing it, in the line of the skin wound, the tumour was laid bare. The outermost part was readily raised up. The muscular tissue of the internal

oblique and the transversalis fascia ran into the inner part of the tumour and were incorporated with its posterior aspect. On cutting the tumour away from its attachments, the peritoneum was exposed, but not opened. Two small vessels on its surface were wounded and ligatured.

The aponeurosis of the external oblique adhered very firmly to the blunt inner extremity of the tumour, but I succeeded in dissecting it back. The superficial epigastric artery was tied and divided. Then I found that this end of the tumour was incorporated with the conjoined tendon and the sheath of the rectus, extending to the anterior part of that muscle. After cutting away the tumour, I trimmed away the outer border of the sheath of the rectus and part of the conjoined tendon, which contained hard, circumscribed deposit.

The wound was united by silkworm-gut sutures, passed through the skin, subcutaneous tissue, aponeurosis of the external oblique, substance of the internal oblique, and transversalis fascia on both sides of the wound. The skin near the outer extremity was united by a horsehair suture, and a drainage tube was passed into the inner angle. The patient made a good recovery. The wound, dressed with iodoform gauze, did not suppurate; the tube was withdrawn by degrees. On the third day the catamenia appeared, for the first time since delivery. On the fourth I removed three of the deep sutures; on the fifth I cut away the remainder. An abdominal belt was supplied, and the patient was discharged in good health on August 1st.

The period ceased again after October 22nd. I saw the patient on January 23rd, 1891; she was pregnant, and the uterus rose to about $2\frac{1}{2}$ inches above the pubes. The cicatrix of the operation was quite healthy, and there was no sign of any recurrence of the growth at the side of the attachment of the conjoined tendon or elsewhere; nor was there any hernial protrusion.

The tumour weighed $5\frac{1}{4}$ ounces, and measured, after removal, 4 inches in length, $2\frac{1}{2}$ from above downwards, and $1\frac{1}{2}$ in thickness antero-posteriorly. On section, its cut surface appeared pale yellow and very fibrous, although not markedly tough; it became convex. Mr. Targett kindly made some microscopical preparations of the tumour for me, at the laboratory of the Royal College of Surgeons. He reported: "The tumour is what the old writers called 'fibro-cellular.' It is composed of young, rather loose

fibrous tissue, in parts indistinguishable from sarcoma, in others denser and much less cellular. I suppose that it may be considered the first stage of the 'recurrent fibroid,' and indicate a guarded prognosis." The tumour and the sections are subjected, this evening, to the inspection of the Society.

This new growth was an example of Sänger's "desmoid tumour of the abdominal wall." Much has been written on the subject by Guerrier,* Sänger,† and Livius Fürst.‡ According to Sänger, the tumour is not very common; considering how much operating is done in the region of the abdomen, we do not find that the most industrious observers succeed in collecting cases by the hundred. It is certain that, compared to uterine myomata and ovarian cystoma, the desmoid tumour of the abdominal wall is relatively rare. Fürst (*loc. cit.*) declares that all cases should be recorded; for much uncertainty still prevails in respect to their nature. He describes a single observation, and, acting on his principle, I follow his example.

One fact in respect to desmoid tumours of the abdominal wall seems clear. They are far more frequent in women than in men; so that the German authorities whom I have already named speak of them as being practically within the domain of gynæcology. In Grätzer's collection of 38 cases, only 5 occurred in men. Guerrier notes but 2 instances of the tumour in male patients, out of a series of 44, which include 27 not in Grätzer's statistics. Eight cases noted by Sänger were all in women. The same can be said of Fürst's case and of my own. Turning to American authors, out of 17 cases under treatment in the United States, as collected by Dr. Edward Ill,§ of Newark, New Jersey, and Dr. Charles Reed, of Cincinnati,‖ only 2 occurred in men. The attention, however, of surgeons who exclusively operate upon women is

* 'Contribution à l'étude des fibromes du tronc en rapport avec la paroi abdominale antérieure,' Thèse de Paris, 1883.

† "Ueber desmoide Geschwülste der Bauchwand und deren Operation mit Resection des Peritoneum parietale," 'Archiv f. Gynäk.,' vol. xxiv, 1884.

‡ "Zur Casuistik der Bauchdecken Tumoren," 'Zeitschrift f. Geburtshülfe und Gynäk.,' vol. xiv, 1888.

§ "Desmoid (Fibroid) Tumour of the Abdominal Walls," 'Transactions of American Association of Obstetricians and Gynæcologists,' vol. i, 1888.

‖ "Fibroid Tumours of the Abdominal Wall," *ibid.*, vol. i. Fink ('Prager medicin. Wochenschrift,' 1890, Nos. 37, 38) states that in six cases under Dr. Gussenbauer's care all but one occurred in women.

keenly directed to the abdomen, and certain circumstances encourage them to record long series of abdominal operations. Hence they are not likely to overlook desmoid tumours. On the other hand, when the same kind of tumour occurs in men, the patients usually come under the care of general hospital surgeons. For many reasons, the cases may then escape record. Hence, perhaps, the disease in question is not so rare in men as the above statistics might lead us to suppose. The surgical registrars of the great metropolitan hospitals are in a position to throw more light on this interesting subject. The position of the tumour bears, apparently, no relation to the sex; in my case it lay low in the left inguinal region, but in Fürst's it was found, when still of small dimensions, close to the right hypochondrium.

Professor Sänger's monograph (*loc. cit.*) is so exhaustive that many more cases must be collected before it can be rivalled, even by an equally capable observer. Some of his conclusions deserve mention and consideration.

It would appear that the desmoid tumour may occur in any of the layers which make up the abdominal wall, although it has never been proved to arise from the parietal peritoneum; and, again, Herzog reasonably suspects that solid tumours in the muscular substance of the abdominal walls are probably sarcomata developed from hæmatomata. This theory, however, agrees with Fürst's view, that desmoid tumours originate in local injuries, as in his own case. These tumours may also arise in the fascia lumborum, and even from the bony and cartilaginous boundaries of the abdomen, the perichondrium and periosteum of the ribs, the costal cartilages, and the pelvic bones. When in the last-named situation they constitute the "fibrous tumours of the iliac crest," of Nelaton, which force their way between the layers of the abdominal wall.

As to the relative frequency of the different seats of origin, statistics do not always furnish us with satisfactory evidence. In 27 cases where the operator took care to note this point, the posterior sheath of the rectus appears to have been the seat of origin of the tumour in 8 cases, including Fürst's, which was published after the appearance of Sänger's paper. In 5, the origin is described as "linea alba, or conjoined aponeurosis"; in 5 as fascia transversalis, in 3 as aponeurosis of the external oblique; in 2 as tendon of origin of rectus. In 2, including my own case, the

tumour apparently rose from the conjoined tendon of the internal oblique and transversalis. One case is noted by Sänger as arising from the anterior sheath of the rectus, and 1 as from the "linea alba and sheath of the rectus," though why this one case is separated from the "linea alba or conjoined aponeurosis" class is not clear, unless Sänger considered that it was dangerous to rely on more than the bare words of the reporter of each case.

I have little, if any doubt, that the tumour in my case arose from the conjoined tendon. It was more intimately associated with that structure than with any neighbouring part. That the inner extremity of the tumour grew into the sheath of the rectus and lay in front of that muscle was to be expected, when we remember the relations of the oblique and transversalis to the rectus in the lower part of the abdomen. I am aware that Solger* does not believe in the accepted opinion that the half aponeurosis of the internal oblique, together with the aponeurosis of the transversalis, passes in front of the rectus at a point marked by the linea semicircularis of Douglas. Anyhow, the tumour in my case, so intimately blended with the conjoined tendon, passed in front of the rectus.

In over 60 recorded cases the desmoid tumour of the abdominal wall was pathologically a "pure, firm fibroma." Sänger boldly makes this statement. It is remarkable that, out of the 27 cases where competent observers described the tumour as "fibro-sarcoma," no less than 2 occurred in men, so little subject to this kind of growth. I agree with Mr. Targett that my own case does not appear to be a pure fibroma. There is more "young" tissue than in Fürst's case (*loc. cit.*, Plate VII). Nevertheless, the appearances which may be noted in the sections in my possession are not positive indications of malignancy.

The cause of desmoid tumours of the abdominal wall is, no doubt, an interesting subject, but it involves pathological subtleties too profound for this memoir. Very clear evidence of

* 'Morphologisches Jahrbuch,' x, p. 102, 1885, and 'London Medical Record,' vol. xiii, 1885, p. 487. Solger insists that the aponeuroses of the internal oblique and transversalis continue behind the rectus to their lowest level, but are very thin there. Above the linea semicircularis they are very stout, for reasons associated with the mechanism of respiration. The abruptness with which the aponeuroses became thin causes the appearance to which the term "linea semicircularis" is applied.

injury has been brought forward, especially in Fürst's case, where the patient was violently pushed against the corner of a table, which caught her below the right hypochondrium, where the tumour soon afterwards developed. In my case there was no history of any kind of injury. I have already referred to Herzog's hæmatoma doctrine. There is no sufficient evidence to prove that these tumours are necessarily developed from some pathological products of injury, clear though Fürst's case may be.

There can be no two opinions about the proper kind of treatment. A desmoid tumour of the abdominal wall should be removed as early as possible. The question remains, When is it too late to attempt its extirpation? This question mainly depends upon one point, namely, how much of the parietal peritoneum can be safely removed. Grant this, and there remains no difficulty about closing the abdominal walls, for a tumour of this class does not tend to adhere to the integument, and the deeper it lies the more chance remains that some of the superficial layers of the parietes, besides the skin, may be saved.

Sänger and other foreign operators have removed enormous desmoid tumours of the abdominal walls, together with the subjacent peritoneum. In one case (Sänger, *loc. cit.*), after dissecting up a flap of integument, the lower part of the tumour was also raised, by an incision below it into the peritoneum. That membrane being found to be inseparable, an aneurysm-needle, armed with a double silk ligature, was passed through the peritoneum and aponeurosis which were to be divided, and brought out again through the same tissues, about an inch further on. A similar transfixion was made an inch external to the former. The process was continued until two complete chains of ligatures were formed, for the ends of the ligatures were tied after transfixion, as in an ovarian pedicle. After each inner and outer transfixion and ligature of the threads, the peritoneum between the inner and outer threads was divided. Thus, when the chains of ligature were complete, the tumour and its peritoneum and aponeurosis were cut away, and there was no loss of blood, either from the tumour during removal, or from the edges of the huge wound in the abdominal wall. By a complicated system of sutures, the integumental flap, including the more superficial layers of the parietes, was fastened down to the edges of the great wound, which were already approximated by silver sutures made fast to plates.

Sutures were passed across the flap so as to unite its deep surfaces as much as possible. Thus a stout pad was formed, resisting hernia, instead of a mere bag of skin, into which three-quarters of the abdominal viscera would ultimately have prolapsed.* Several drainage tubes were passed across and along the wound. The patient, in this case, did well.

In different cases of large tumours of the abdominal wound, the procedure must vary. The double chain of ligatures, to anticipate hæmorrhage, is a good idea, but plenty of pressure forceps, such as most British operators possess, will allow the surgeon to dispense with the inner chain. The formation of a good stout pad of integument is an excellent principle.

The apposition of a raw flap of integument to the viscera, in place of the parietal peritoneum, appears perfectly justifiable and free from any danger of adhesions, as long as the surgeon takes antiseptic or "aseptic" precautions. Dr. Kelterborn, of Dorpat, has recently shown that the stripping off of peritoneum is not sufficient for the development of an adhesion,† and that endothelium will develop over a considerable area of tissue, whence the serous membrane has been removed; although perfect repair must be impossible in an extreme case, like Sänger's.

The peritoneum should always, however, be saved whenever possible. In my own case it was preserved. I tied the wounded vessels on the peritoneum with caution, as I feared damage to the serous membrane, which might have caused sloughing. The intestines bulged freely under the exposed peritoneum, and had not Mr. Stormont Murray kept the patient very thoroughly under chloroform, I might have had some difficulty in applying the sutures without damage to the viscera. The sutures were passed through all the layers of the parietes superficial to the peritoneum; the process proved harder than in ovariotomy, when, the serous membrane being divided, the passage of the needle can be more thoroughly observed. The depth of the sutures ensured a strong cicatrix.

* These complicated manipulations are demonstrated in the fine drawings illustrating Dr. Edward Ill's monograph (*loc. cit.*).

† "Versuche über die Entstehungsbedingungen peritonealer Adhäsionen nach Laparatomien," 'Centralblatt f. Gynäk.,' No. 51, 1890. A summary of this paper will be found in the 'British Med. Journal,' vol. i, 1891, p. 294.

Sir Hugh Beevor asked Mr. Alban Doran whether he did not think the section of the tumour under the microscope showed no evidence of sarcoma, but consisted solely of fibrous tissue and very young connective tissue cells, in fact that the tumour was of pure fibrous tissue.

Dr. Cullingworth enquired whether Mr. Doran could furnish any absolute test by which fibro-sarcomata can be distinguished histologically from fibro-myomata, and even from certain pure fibromata in which there is an abundance of young tissue. Or was it still the fact that the classification must rest chiefly upon clinical evidence? Would Mr. Doran kindly say how long it was after the operation when the patient was last seen and found free from recurrence, for this would probably settle the question of diagnosis, in this particular case, so far as sarcoma was concerned.

Mr. Alban Doran said, in reply to Mr. Sutton, that the term "desmoid" had been revived by Sänger to signify that kind of abdominal tumour which was exhibited this evening. The term was clinical, not pathological. Mr. Doran, when he first saw the case, believed that the tumour had probably developed in the left round ligament. The relations during operation, however, showed that it had no connexion with that ligament, which ends in the connective tissue of the pubes, and not in the conjoined tendon whence the tumour arose. In reply to Sir Hugh Beevor, he thought that the tumour was essentially fibrous tissue with very young elements, and hardly malignant. The old terms "desmoid" and "fibro-cellular" were freely used to signify soft fibromata, œdematous fibromata, myxomata, and myomata. In reply to Dr. Cullingworth, Mr. Doran stated that the tumour was nearer to the fibro-sarcoma than the fibro-myoma type. He had seen the patient in March and there were then no signs of recurrence. He was interested in Mr. Haslam's testimony as to the rarity of desmoid tumours of the abdominal wall. Sir Spencer Wells had removed a true fibro-myoma of the round ligament, now preserved in the museum of the College of Surgeons (Path. Series, 4644). Mr. Doran repeated his challenge to the surgical registrars of general hospitals, and hoped that, at least, some industrious pathologist would communicate with the registrars with a view of ascertaining if these tumours were as rare in male subjects as was usually supposed.

THREE CASES OF STRANGULATED HERNIA OF THE CÆCUM IN INFANTS.

By Stephen Paget, F.R.C.S.

It is an old saying that if a man gets one curious case he is sure to have another like it soon. By an odd chance of this kind, I have had in the last three months three cases of strangulated hernia in infants; and all three were cases of hernia of the cæcum. Mr. Howard Marsh, in the tenth volume of the St. Bartholomew's Hospital Reports, has collected forty-seven cases of strangulated inguinal hernia in children. In thirty-four the sac was opened at

the operation. Out of these thirty-four, only one was a hernia of the cæcum. As regards the anatomy of the parts concerned in hernia of the cæcum, it has been described by Mr. Treves, in the tenth volume of the 'Transactions' of this Society, and by Mr. Lockwood, in his lectures on "Hernia." They draw attention to two facts:—1. The existence of a proper sac in these cases. 2. The existence of a fold of peritoneum, containing muscular fibres—the plica vascularis—which passes down from the cæcum along the back of the sac, and helps to cause the hernia by drawing or guiding the cæcum into the sac. Both these observations are illustrated by my cases, which are as follows:—

Case 1.—On January 27th, a male infant, 1 year old, strong and well nourished, was admitted to the West London Hospital with strangulated right scrotal hernia. The rupture had been noticed soon after birth; the child had worn a truss. The day before admission the rupture came down; the child vomited several times and had difficulty in micturition. On admission it looked well, and was not in pain. The hernia was large and tense; there was no impulse; the scrotum was red and inflamed. I operated at once. There was a proper sac; it contained a loop of small intestine and some omentum, and above these, at the neck of the sac, was the cæcum. There was no fluid in the sac. The intestine looked bruised, but was not discoloured. I divided the stricture and reduced the cæcum, and then the small intestine ran back of itself. The bowels acted twice that evening, and the child did well.

Case 2.—On February 11th, a male infant, 3 months old, was admitted to the same hospital with strangulated right scrotal hernia. The rupture had been noticed three weeks after birth; the child had worn a worsted truss. The day before admission the rupture came down; there was repeated vomiting, and retention of urine. It was a very small baby, a seven months child, and the thirteenth in the family. It looked free from pain, and was not collapsed. The hernia was large and tense; there was no impulse. I operated at once. There was a proper sac, with about a drachm of clear colourless fluid; in it was the cæcum. The vermiform appendix, which was unusually long, came forward from the lower aspect of the cæcum. The cæcum was very slightly congested. I divided the stricture, letting out some more fluid. Then the infant's legs were held up high, and the cæcum was reduced. The bowels acted a few hours later. The wound healed, and the bowels acted regularly; but the infant had been brought to the hospital in the middle of a frosty night, and four days after the operation it showed signs of acute bronchitis, and of this it died on February 23rd, twelve days after the operation.

Case 3.—On December 18th, a male infant, 3 months old, neglected and starved, was admitted to the same hospital with strangulated right scrotal hernia. There was vomiting, and some retention of urine. The hernia was large and tense, and without impluse; there was acute inflammation of the scrotum. With chloroform, I reduced the hernia, and could now feel that the testicle and the cord were greatly swollen. After this all went well for eleven days; the bowels acted freely and regularly; the

infant took a quart of milk daily, and gained flesh. But on December 29th the scrotum was again inflamed, and on the 30th it was acutely inflamed and distended, and resonant on percussion; the child vomited once; the bowels acted freely. On December 31st the infant was restless; it vomited twice; the bowels acted freely. The scrotum was intensely inflamed, and at one spot, the size of a sixpence, it was thinned and breaking down. It was resonant on percussion. The prepuce was œdematous; the penis was twisted. In the inguinal canal was a hard cylindrical swelling; the spermatic cord could not be felt. Under chloroform, the swelling was partly reduced, with a loud gurgling sound; the scrotum was now less tense, and the testicle and the cord could be felt again. But in a couple of hours things were as bad as ever: the œdema of the prepuce was increased, and there was distinct emphysematous crackling at the bottom of the scrotum. It was plain that the bowel had given way. I cut down carefully, and found no distinct sac. The bowel and the sac were all matted together; at the lower part of the bowel was a ragged opening; the tunica vaginalis was full of fæces, and the testicle was acutely inflamed. I passed a director in at a pinhole opening which I had made in the bowel during dissection, and out at the ragged opening lower down, and laid the two openings into one, and stitched the bowel in the wound. For about a month the infant did well; the cavity of the tunica vaginalis granulated up, the bowels acted twice daily through the wound, and there was never any great prolapse. But at the end of a month the infant began to lose flesh, at the rate of a quarter of a pound a week, and it died between five and six weeks after the operation.

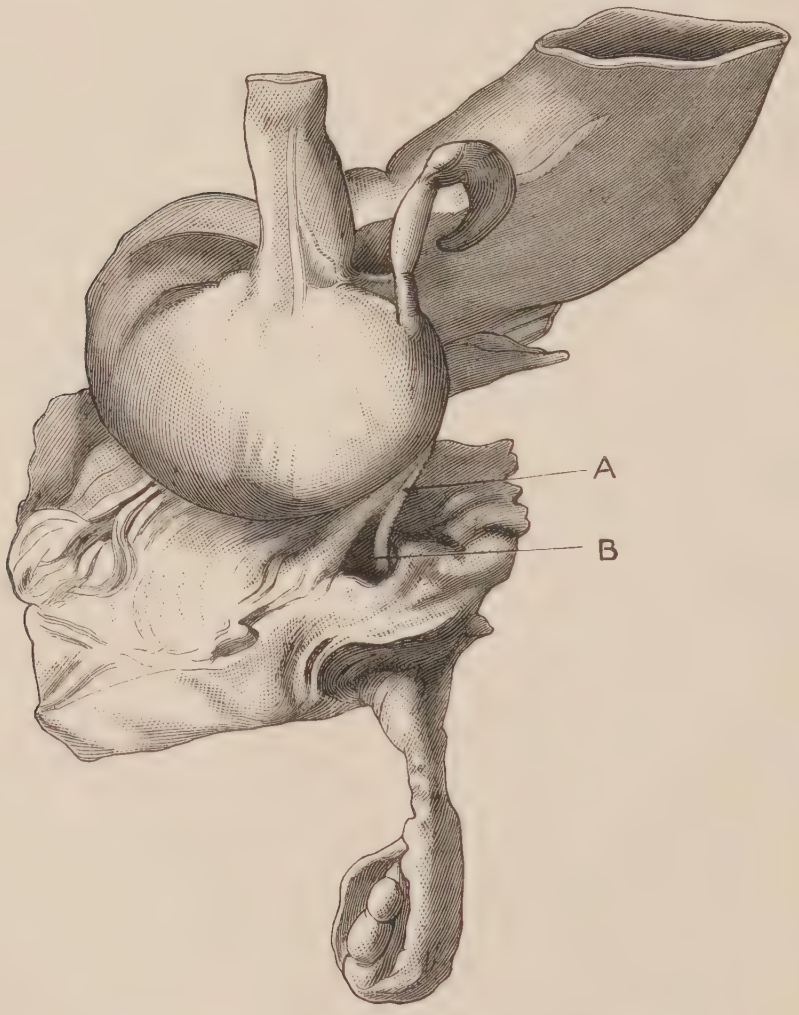
These cases seemed to me worthy of the notice of this Society, because strangulated hernia seldom occurs in infancy. Mr. Marsh says, "Many surgeons have passed through a long career of hospital practice without meeting with any instance of it, and I believe the only case in which an operation has been performed at the Hospital for Sick Children in the course of the last twenty-three years is that which I have just recorded."

In a very large majority of Mr. Marsh's cases the hernia was congenital; indeed, so far as the notes tell us, only four were not congenital. The hernia was more frequent on the right side than on the left, in the proportion of three to one. As regards the contents of the sac in Mr. Marsh's cases, the sac was opened in thirty-four out of the forty-seven cases. Of these thirty-four cases one was a hernia of the colon, one was a cæcal hernia on the left side of the body, and one was a hernia of the sigmoid flexure; and two were cases of hernia of the ovary.

As regards the symptoms of strangulated hernia in infants, I may point out that in my cases there were two symptoms which we seldom find in adults. One of these was the retention of urine; the other was the acute inflammation of the skin of the scrotum.

Of my three cases, the first made a good recovery. The second case recovered so far as the hernia was concerned. But it was a seven months child, and the thirteenth in a poor family; it was brought to the hospital in the middle of a cold night, and I am sorry to say that after the operation it was put in a cot near a window, instead of in a basket near the fire. From these causes it caught the bronchitis of which it died. The specimen (Fig. 1) from this case shows the plica vascularis passing down into the sac. The sac is contracted. There is no communication between the

Fig. 1.



A, Plica vascularis. B, Mouth of sac, contracted.

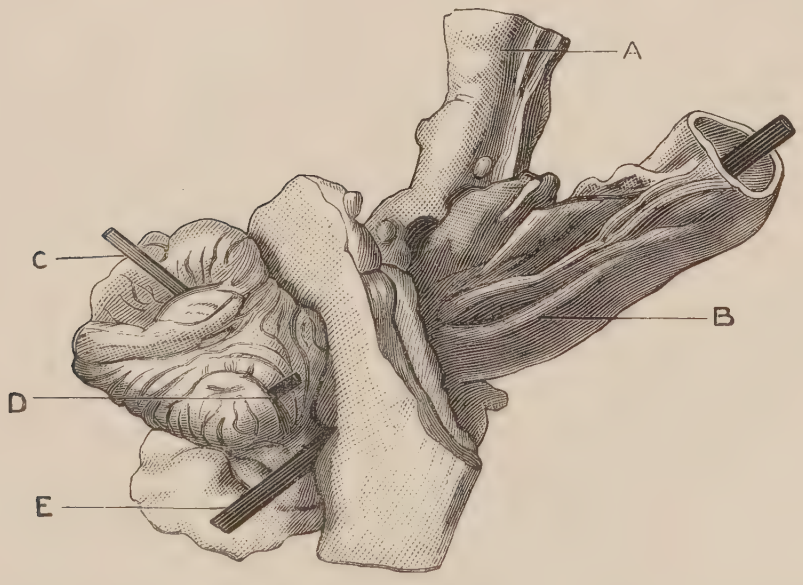
sac and the tunica vaginalis. The great length and free mobility of the vermiform appendix suggest that it may have made its way first into the sac, and so may have drawn the cæcum after it; and we

know that cases have occurred where the appendix alone was found in the sac at the time of operation.

In the third case I cannot doubt that I really reduced the hernia when the child was admitted. There was distinct slipping back of the bowel under my fingers, with a loud gurgle. The vomiting stopped for eleven days; the infant took a quart of milk daily; the bowels acted freely and regularly. They continued to act freely up to the time of operation, when the bowel had actually burst into the tunica vaginalis. It is therefore impossible to say exactly at what time the cæcum came down again and became strangulated.

As regards the treatment of this case, it appears from Mr. Marsh's paper that it would have been better to have done less. He gives two cases. In one of them the hernia had been strangulated for many days; the scrotum had given way, and there was a free discharge of fæces. No operation was done, the openings in the scrotum gradually closed, and the child recovered. In the other case the bowel was found to be gangrenous, and firmly adherent to the sac. It was poulticed, and came away in a few hours. Twelve days later the bowels acted the right way, the wound closed, and the child recovered. Perhaps therefore it would have been better in my case to have done nothing, or to have simply incised the

FIG. 2.



A, Large intestine. B, Small intestine. C, Rod passed into large intestine. D, Rod passed into vermiform appendix. E, Rod passed into small intestine.

scrotum. There might thus have been no prolapse of the bowel. On the other hand, as it was a hernia of the cæcum, the continuity of the bowel would perhaps not have been restored as it was in the cases quoted by Mr. Marsh. The specimen (Fig. 2) shows the prolapsed cæcum, of a curious hammer-headed shape. A very fine rod is passed into the everted mouth of the vermiform appendix. A rod is passed into the ascending colon, which is contracted from disuse, and a rod is passed along the ileum.

In all three cases there was a complete hernial sac. In the third case this, of course, could not be made out at the operation, but it is clearly seen in the specimen. As regards the question whether in these cases there was any communication between the sac and the tunica vaginalis, there was no communication in the first two cases; the third case is of course uncertain.

I wish, in conclusion, to express my thanks to Mr. Eccles, the house surgeon, for his constant care of the children, and for his help in putting these notes together.

ON CHRONIC ATROPHIC RHINITIS (OZÆNA).

By F. de Havilland Hall, M.D., F.R.C.P. Lond.

Mr. President and Gentlemen,—It is well from time to time to take a disease of frequent occurrence, to examine it from all points of view, and to inquire into the most approved methods of treatment. I have chosen for the subject of my paper to-night "Chronic Atrophic Rhinitis or Ozæna," and I trust that it will give rise to a useful discussion. No society is better qualified to express an opinion on this disease than the Medical Society, which numbers among its Fellows so many men engaged in general practice, and who consequently have an opportuity of watching the course of a chronic affection; whereas men chiefly occupied in special practice only see fragments of the case. Moreover, I do not think that ozæna has been discussed in London since the meeting of the International Medical Congress in 1881. As will be seen in the course of my paper, my remarks are entirely confined to the fœtid discharge met with in chronic atrophic rhinitis, and that ozæna is used as a synonym. It would be better, however, to discard the term ozæna altogether, as it is very loosely employed. Chronic atrophic rhinitis is distinctly a disease of early life. Zaufal even

regards it as being due to a congenital defect. The malady, however, seldom appears in subjects under 4 or 5 years of age; towards puberty it increases in severity, and the majority of cases commence before the 16th year. Schech has seen only two cases which developed after the 20th year, once in a highly chlorotic lady after the removal of numerous polypi which obstructed the nose, and secondly in an elderly married woman much pulled down by a uterine disorder.

Sex seems to exercise considerable influence, as females are much more frequently attacked than males, in about the proportion of seven to two.

There is a considerable divergence of opinion about the general health of the subjects of this disorder. Bosworth says that it occurs as a rule in persons enjoying perfect health. On the other hand, Michel states emphatically that no one who is quite healthy gets ozæna, and Schech does not remember a single case of genuine ozæna in which the patient did not exhibit traces of anæmia, chlorosis, scrofula, or tuberculosis. The latter view is certainly in accord with my own experience, as I have seen an undue proportion of cases in phthisical and anæmic patients, and I am inclined to believe that in anæmic young females there is a peculiar vulnerability of mucous membranes generally, their well-known liability to gastric ulcer being an example of this tendency. It has been stated that inherited syphilis may act as a predisposing cause to atrophic rhinitis, but I have no personal experience on this point. That there is an hereditary tendency to this condition is shown by the fact that atrophic rhinitis is frequently met with in more than one member of the same family.

One of the earliest questions to be settled in the pathology of atrophic rhinitis is whether the disease is usually preceded by a hypertrophic stage. There is very little positive anatomical evidence either one way or the other, but on general pathological grounds it is easier to understand that the disease comes on as a result of a catarrh attended with hypertrophic changes at the commencement than to believe that it is *ab initio* of an atrophic character. Moreover, cases have been recorded, and I have seen at least one myself, in which an atrophic condition was seen on one side and a hypertrophic on the other. Bosworth is of opinion that atrophic rhinitis develops as the result of a purulent rhinitis occurring in children.

Zaufal regards the disease as being due to a congenital deficiency in the turbinated bones; another theory is that of an arrest in the development of the turbinated bones. Both these theories are based on the fact that in atrophic rhinitis the nasal passages are much increased in size, and that as a result of the diminished force of the current of air passing through the dilated passages mucus tends to be retained and undergo decomposition. To these theories it may be replied that the diminution in the bulk of the turbinated bones is a part of the general atrophy of the structures of the nostrils, which is so characteristic a feature of the disease, though it is met with in an advanced state, only when the symptoms have already been well marked for some years. In favour of the view that the disease is due to an enlargement of the passages, which allows the mucus to be retained and undergo decomposition, is the fact that a case of ozæna has been recorded as having promptly followed upon the eradication of a nasal sarcoma, which had caused dilatation of the cavity, also Schech's case already mentioned. Michel has put forward the view that a chronic catarrh of the sphenoidal and ethmoidal sinuses is essential to the existence of ozæna.

The remaining point to be discussed in the pathology of atrophic rhinitis is the action of micro-organisms. Loewenburg has described a micrococcus which he discovered in the mucous membrane, and which he regards as the cause of the disease. This view has been supported by many writers, but it is far from being generally accepted; on the contrary, most authorities regard the micro-organisms as being the result, and not the cause, of the disease. Marsano, for example, has tried inoculations with the micro-organisms which he regards as the cause of the disease, but without producing ozæna. There is, however, no doubt that bacteria play an important part in the production of a fœtid odour. Hajek has discovered in ozæna a short bacillus occurring in the form of a diplococcus or in chains, which possesses the property of decomposing organic substances with the formation of a penetrating stink. To this organism Hajek has applied the name *Bacillus fœtidus*, and he thinks that it, as well as the other bacteria in ozæna, only causes a part of the disease.

The changes that have been met with on post-mortem examinations show extreme atrophy of the mucous membrane and bony structures. Krause lays great stress on the fatty degeneration

which he found in the mucous membrane, and in the gland epithelium, and attributes the sickening and rancid smell which is so marked a feature of the disease to the enormous quantity of fat secreted into and beneath the epithelium, which quickly undergoes decomposition, liberating fatty acids. More recently, Habermann has confirmed part of these observations. From a very exact microscopical examination of two cases, he concludes that the nature of ozæna consists in a fatty degeneration of the acinous glands of the nasal mucous membrane, and of Bowman's glands, along with an inflamed and infiltrated mucous membrane. The fibrous degeneration of the neighbouring membrane and the atrophy are caused by this condition, inducing reaction in neighbouring parts.

From a consideration of the anatomical changes met with in the disease, it will be at once seen that the degenerate mucous membrane is no longer capable of secreting the bland sero-mucous fluid which it is its function to furnish, but secretes instead a muco-purulent discharge which readily dries and forms the crusts. These are retained in the nasal passages and, undergoing decomposition, give rise to the excessively offensive odour whence the disease derives its synonym of ozæna. The odour is of a peculiar sickening character, and when once it has been experienced it can readily be recognised again. Fortunately for the patient, the sense of smell is early lost in atrophic rhinitis, so that he is in happy ignorance of the disgusting smell emitted from his nostrils. At the menstrual periods there is generally a great increase in the severity of the symptoms, and cases which under treatment have become free for a time from smell may again be offensive. After 20 the smell usually lessens, and it may entirely disappear in old age.

In some cases of ozæna it will be found that there is a double source of the fœtor of the breath, *i.e.*, that it proceeds both from the nostrils and also from the trachea. The tracheal condition is secondary to the rhinitis, but when once it has started it may continue independently. The diagnosis of tracheal ozæna rests on the expectoration of greenish, thick, viscous pellets, having the odour of ozæna, occurring especially in the morning; the persistence of fœtor of the breath after the nostrils have been thoroughly disinfected, and, finally, on the fact that the air exhaled by the mouth is as fœtid as that from the nostrils.

The dry condition of the nose in atrophic rhinitis causes it to be irritable, so that the patient is inclined to pick or scratch the interior, and thus cause excoriation of the mucous membrane and slight hæmorrhage. Indeed, the constant introduction of the finger up the nostrils may give rise to a perforation in the septum, the tissues being already thinned by the disease itself. In consequence of the dilated and dry condition of the nasal passages the inspired air does not get filtered, warmed, and moistened, so that pharyngeal, laryngeal, and bronchial catarrh are frequently met with in patients afflicted with this disease. One of the most troublesome symptoms is the hacking cough produced by the dry condition of the pharynx, and the hawking induced by the patient's attempt to dislodge the dry crusts which adhere to the naso-pharynx.

Ear and eye troubles are frequently met with in cases of atrophic rhinitis. Among the former are to be mentioned acute and chronic catarrh of the middle ear and tinnitus; among the latter, ulcer of the cornea with hypopion and conjunctival catarrh. Many reflex symptoms also occur in the course of the disease, such as paræsthesia in the pharynx and larynx, headache, neuralgia, giddiness, &c. I am inclined to believe that dyspeptic symptoms are sometimes due to ozæna, at all events I have seen great improvement in the digestive system after methodical treatment of the nose.

Attempts have been made to divide cases of atrophic rhinitis into two classes, the dry and the moist forms, but I do not think that any advantage is gained by this sub-division. At one time dry crusts will be found, and at another time there will be a puriform discharge from the nose. The condition of the atmosphere has some effect on the nature of the secretion. The appearance of a patient suffering from atrophic rhinitis is very suggestive, the nose being depressed at the bridge, giving rise to the condition termed saddle-back, and the tip turned up, showing the nostrils, which are unusually dilated.

By anterior rhinoscopy in a typical case the nostrils will be seen filled with dry crusts, consisting of inspissated muco-pus and stinking abominably. When these have been removed the cavity of the nose will be found greatly dilated, so that it may even be possible to see the posterior wall of the pharynx. The mucous membrane is usually pale, but sometimes it is slightly reddened. Distinct ulceration is unusual, though the detachment of the crusts may

give rise to a little bleeding. If the cartilages and bones are necrosed the case can no longer be regarded as one of genuine atrophic rhinitis. The turbinated bones may be so atrophied as hardly to project into the cavity.

By posterior rhinoscopy a similar condition of atrophy can be recognised and the posterior nares may be found occluded with dry crusts. The pharynx is dry and glistening, or covered with adherent mucus, which in towns is usually black from soot or other impurities in the air, the capacious nostrils having failed to arrest them. A dry, glazed state of the pharynx or the presence of adherent mucus should suggest the idea of atrophic rhinitis.

The diagnosis of atrophic rhinitis is seldom a matter of any difficulty; the characteristic stench emitted by the patient and the dilated nasal passages with dry crusts of mucus are not present in other diseases. Suppuration in the antrum might possibly be mistaken for atrophic rhinitis, but in this condition the discharge is confined to one nostril, and, as Christopher Heath points out, "the offensive smell is perceived only by the patient, and not by his friends, the reverse being the case in ozæna; and, again, the discharge is only occasional, is determined by the position of the head, and is simply purulent, whereas in ozæna the discharge is constant and mixed with offensive crusts of the nasal cavities." Syphilitic disease of the nasal passages might be mistaken for atrophic rhinitis, but in this case the discharge has not the characteristic smell of the latter disease, and manifestly depends upon ulceration of the mucous membrane and necrosis of the subjacent bone. Moreover, the ready manner in which it usually yields to treatment will assist, if there be any doubt, in the diagnosis.

It is hardly necessary to warn against confounding the fœtid discharge due to the presence of a foreign body in the nostril with atrophic rhinitis. As it is often impossible to get any history of the introduction of a foreign body, the diagnosis must rest upon a careful rhinoscopic examination.

Though atrophic rhinitis does not in any way threaten life, it may make it almost unendurable and prevent the sufferer from being able to gain his livelihood on account of the nuisance he is to all around him. Schech speaks of the prognosis as being absolutely hopeless, and Fraenkel avows that a cured ozæna is unknown to him. Others maintain that a cure is possible in children, and Noquet thinks that the atrophic mucous membrane may even be

regenerated! I am in the habit of telling patients that, though a complete cure is not to be expected, if he will only take the necessary trouble the disease will be robbed of its worst features.

Though local treatment is all important in atrophic rhinitis, it is well before commencing it to see if there be any underlying general condition of ill-health which needs correction. If anæmia be present the combination of 10 grains of the ammonio-citrate of iron with 3 minims of arsenical solution given thrice daily exercises a beneficial effect. If there be any tendency to phthisis cod-liver oil should be administered. The iodides do not seem to have any good effect in the ozæna due to atrophic rhinitis. The essential point in the local treatment is the thorough cleansing of the nasal cavities. This may be effected in three ways:—(1) by means of the douche; (2) by the use of the anterior and posterior syringe; (3) by spraying. At one time I used the douche almost exclusively, but of late years I have entirely given it up. Several cases of otitis media due to the fluid passing up the Eustachian tube while the douche was in operation have been recorded, and though I had had no personal experience of this accident it was one of the reasons which induced me to discontinue this form of treatment. Pins' modification of the douche to which Dr. Ball has recently directed attention seems to obviate this objection. The anterior and posterior syringe answers fairly well in the less severe forms of the disease, but where crusts have collected on the sides and roof of the nasal passages the only effectual method of washing them away is by means of a spray emitted with considerable force from the apparatus. For those who prefer the douche or the syringe the following solution may be employed.

℞ Sodii bicarb. } ʒj;
Boracis, āā }
Glycerini acidi carbol., ʒij;
Aquam, ad Oss.

This is known as Dobell's solution.

Bayer, however, states that carbolic acid is not to be recommended locally because it often completely destroys the power of smelling in those cases in which it has been preserved. Chlorate of potassium or liquor potassii permanganatis may be substituted for the borax. Great care should be taken that the solution used should be of a suitable temperature.

I have found "Listerine," an American preparation containing thyme, eucalyptus, and other essential oils, together with benzoboracic acid, a most serviceable and pleasant disinfectant. It may be conveniently employed in Dobell's solution instead of the glycerine of carbolic acid in proportion of one or two of listerine to ten of the lotion. As on account of the chronicity of the disease the employment of expensive drugs, or any drugs at all for the matter of that, is a matter of serious consideration to people of limited means, it is often necessary to use the cheapest preparations. For this purpose common salt, 2 drachms in half a pint of water, or chlorate of potassium 1 drachm, or liq. potass. perm. 1 drachm in same amount of water will be found very useful.

My mode of treatment is first to soften and loosen the crusts as much as possible with a simple saline spray. For this purpose I usually employ bicarbonate of sodium, chloride of sodium, borax, of each 30 grains, white sugar, 45 grains, in half a pint of warm water. This is practically the formula introduced by Morell Mackenzie. If, on rhinoscopic examination, hardened masses still adhere to the mucous membrane they may be cautiously removed by means of the nasal forceps, or by the use of the anterior or posterior nasal syringe. The patient should be told to use the spray at least night and morning—sometimes three times a day may be necessary—and to come again in a week's time. If on the second interview the nostrils are found to be free from crusts and fairly sweet the patient is to be directed to continue the same treatment. If crusts are again met with they should be removed as on the first occasion. The simple saline spray may be continued for some weeks, and if there is no evidence of distinct improvement it will be well to make a change. I have found the addition of listerine to the alkaline spray useful in diminishing the fœtor from the nose and acting as a mild stimulant. If a more powerful disinfectant is required Dobell's solution may be used, and the amount of carbolic acid increased if necessary, also weak solutions, 2 to 5 grains, of sulphate of zinc, alum, or nitrate of silver will at times be found useful. The permanganate of potassium formula or creolin 1 to 500 up to 1 to 100 is useful as a change. After thorough cleansing of the nostrils insufflation may be employed. For this purpose I have used iodoform, iodol, or boric acid. Recently aristol (the iodide of thymol) has been highly recommended. Many other combinations have been advised, but what-

ever is used it must be distinctly understood that it is not so much the particular solution employed as the thoroughness with which the process of cleansing the nostrils is carried out which conduces to the cure. Again, it is very desirable to ring the changes on the solutions employed. After thoroughly spraying out the nasal cavities I have found smearing the interior with oil of eucalyptus and vaseline (a drachm to the ounce) serviceable, as it appears to prevent the mucous membrane becoming dry. The vaseline should be warmed and applied by means of a camel's hair brush. Another plan is to spray out the nostrils with one of the liquid paraffins, *e.g.*, paroleine.

In intractable cases Gottstein's tampon is very useful. To obtain the best effect the plug of cotton wool should be in contact with the whole of the interior of the nostril. The plug acts as a stimulant to the mucous membrane and so keeps up a certain amount of watery discharge, thereby preventing the formation of crusts.

Rualt recommends the following method of treatment :—

The nostrils are to be washed out with a solution of bicarbonate of sodium. They are then to be painted with naphthol-camphor, which consists of one part of naphthol to two of camphor with a variable quantity of fluid vaseline according to the sensitiveness of the patient. The drying of the mucous membrane is prevented by frequently spraying the nostrils with fluid vaseline with the addition of naphthol-camphor (1 to 10,000 up to 1 to 1,000). Later on, to replace washing out by means of inhalation of vapor benzoini.

Resorcin, the galvanic cautery, the constant current, and innumerable other plans of treatment have been suggested; the very number of drugs recommended being a proof that no one of the number can claim to be a specific. I have, however, I think, sufficiently described the best plans of treatment in vogue, and I need only add that I shall be extremely thankful for some new hints for combatting this troublesome affection.

Mr. Spencer Watson quite agreed with Dr. De Havilland Hall that the subjects of "ozæna" often had a family taint of tuberculosis, and was of opinion that the chronic atrophic rhinitis was of the nature of "lupus erythematosus." The clinical history and the histological structure both favoured this view. He could not agree, however, that the douche should be entirely set aside. In some cases it was impossible to remove the adherent crusts without the free use of the douche. When, however, the douche caused pain in the frontal region, it should be discontinued,

and a spray containing iodine and carbolic acid employed. One case of cure only had been recorded, viz., that of a girl the subject of ozæna whose nostril was accidentally inoculated with pus from a gonorrhœal discharge.

Dr. DE HAVILLAND HALL said that if atrophic rhinitis were allied to lupus erythematosus it was curious that no instances of the co-existence of the two diseases in the same patient had been noted. He had not tried Koch's treatment in atrophic rhinitis, but at the present moment he had a patient with strumous rhinitis who was being injected with Koch's fluid with an excellent result. He could not agree with Mr. Spencer Watson that the douche was necessary; if the medical man himself thoroughly cleansed the nose at the commencement of the treatment the patient could be instructed how to prevent the crusts accumulating again.

April 13th, 1891.

THE IMPORTANCE OF THE POST-PROSTATIC (OR TRIGONAL) POUCH IN THE SURGERY OF VESICAL CALCULUS, ILLUSTRATED BY SEVERAL CASES.

By G. BUCKSTON BROWNE.

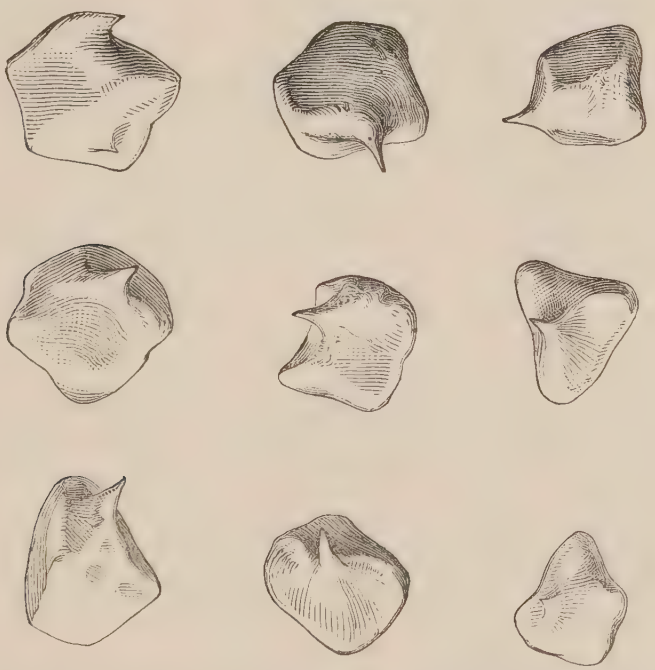
THE surgery of vesical calculus may be considered under two heads: first, the finding of the stone, and, secondly, its removal. Errors in sounding for stone have perhaps attracted more attention than mistakes in the diagnosis of any other surgical malady, and the best way of removing a stone from the bladder is even still a frequent matter for argument and difference of opinion. The chief cause of all this difficulty in diagnosis and difference of opinion as regards the method of removal is the fact that the bladder is not always a simple sac, but sometimes has pouches, pockets, or sacculi opening out of its general cavity, in which stones may lie, and escape the diagnostician's sound or the lithotrite, tube, or forceps of the operator. All this is simple enough and known to every surgeon, but the fact that vesical sacculi are of very different kinds is not universally known or appreciated, and great error is caused by speaking loosely of pouched or sacculated bladders, as if all the pouches of a bladder were alike. Bladder pouches may be divided into three varieties:—1. There is the well-recognised sacculus which we may call the ordinary

sacculus, consisting of a protrusion of mucous membrane between the muscular fibres of the bladder. Such sacculi are found in the upper part, at the side, and in the floor of the bladder; and where this is covered with peritoneum they, too, are covered by the same membrane. 2. A pouch often forms behind the trigone—*i.e.*, behind a line drawn between the vesical orifices of the ureters. It is part of the general cavity of the bladder, but is sometimes deep enough to make the discovery of a stone lying within difficult. This may be called the post-trigonal pouch. 3. There is a post-prostatic or trigonal pouch to which I would particularly draw attention, because I believe it has hitherto received little notice, and because I think it is the chief cause of error in searching for stone and of imperfection in its removal, whether by the lithotrite or by the knife. The post-prostatic pouch is often of extraordinary depth. If the patient be supposed to be lying down, it may be said to consist of the trigone of the bladder, pushed down between the enlarged and projecting prostate in front and a thickened and firm inter-ureteral ridge behind. Where there is much intra-vesical prostatic projection, the pouch may literally be roofed over by this prostatic outgrowth. Calculi in this pouch cause much pain, since the trigone of the bladder has a larger nervous supply than any other portion. Lying as the pouch does in front of and below the orifices of the ureters, it is a perfectly contrived trap for the catching and retention of renal calculi upon their entrance into the bladder, and it will readily be seen how favourably the stones are there placed for growth. That they do lie there and grow into extraordinary shapes is shown by several of the stones about to be described. The object of this paper is to put forward the following two propositions for consideration, and to relate illustrative cases. 1. That, owing to the post-prostatic pouch, vesical calculus is often difficult to detect in cases of enlarged prostate, and that if the pouch is deep it may be absolutely impossible to find the stone by any instrument passed in through the urethra. 2. That, owing to the occasional depth of the post-prostatic pouch, lithotrity is sometimes an impracticable operation, it being mechanically impossible to evacuate the bladder of all *débris* by urethral instrumentation.

Case 1.—D. T——, aged 68, seen with Dr. Whittle, of Brighton, July, 1886. For seven years all urine by catheter; urine very fetid. By rectum, prostate found very large. A tight urethral stricture at

5 inches from meatus; urethra 9 inches long. Owing to the stricture, catheterism very difficult. I therefore performed internal urethrotomy. After this no difficulty; passed Nos. 13 and 14 with ease.—February 14th, 1887: Patient complains of a pricking pain at the mouth of the bladder; worse when he rides. Sounded him and found a small stone.—February 19th: Lithotrity; a most tedious operation. It seemed that as soon as one calculus was crushed and cleared out the tube and aspirator would suck out another stone from some sac or pocket, for immediately after the *débris* were withdrawn a stone would rattle against the end of the tube, rendering the re-introduction of the lithotrite necessary. This was repeated over and over again, and I entered in my notes at the time that the bladder must be sacculated. All went well, however, and in July, 1888, I sounded the patient and found nothing. I saw nothing more of him for twelve months, when on June 3rd, 1889, he came up with symptoms which induced me to sound him again, and a stone was found. On June 8th, finding much difficulty in the introduction of instruments, owing to the stricture, and thinking from the sensation my sound gave me when in contact with the stone that it was not a case of calculus free in the cavity of the bladder, I determined to open the bladder above the pubes, and found a large intra-vesical prostatic lobe, and below it a pouch almost entirely cut off from the cavity of the bladder, and packed with stones. They were removed one by one, nine altogether, and each bore one curious spike. These spikes, well shown in the engraving (Fig. 1),

Fig. 1.



Nine calculi, each bearing a spike, probably the result of friction from being closely packed together in a post-prostatic pouch.

may simply have resulted from friction, but of this I am not perfectly satisfied. The wound was healed in a fortnight, and on July 12th the patient went home, passing his catheter every five hours, and entirely free from pain. During the summer he walked and rode, and swam in

the sea. On July 30th he wrote: "Never better for years. Can cross my legs now, which I could not before, and can sit in any position." On September 13th he came up to London in a dying condition. He had had a long swim, which had been followed by a rigor and severe left renal colic. He died next day, and on the 15th I made a post-mortem examination. The prostate was found projecting into the bladder, as already described, and under it there was an enormous pouch containing one calculus. This calculus was round, had no spike, and was entirely unlike those removed by the supra-pubic operation. Both ureters were dilated, and the left kidney was full of large calculi. It was breaking down into abscess. Fig. 2 shows the prostate and bladder. The prostatic projection into the bladder (D) will be readily noted; under it lie

FIG. 2 and 3.

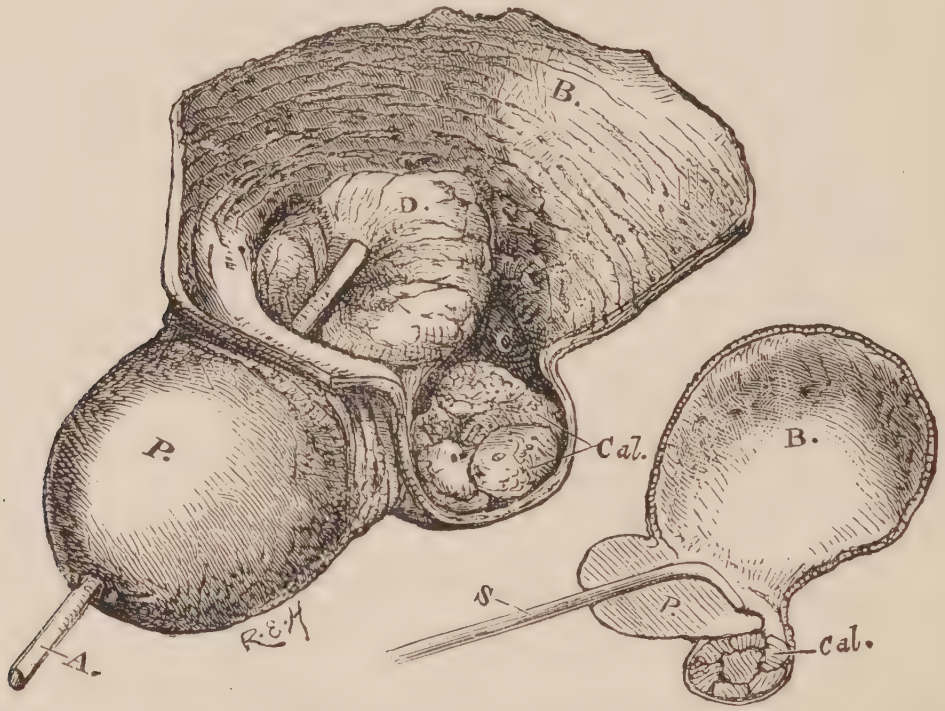


FIG. 2.—A, Piece of bougie showing course of prostatic urethra. B, Bladder wall, behind the inter-ureteral ridge (C). Cal., The nine spicate calculi, in post-prostatic pouch. D, The intra-vesical prostatic growth. P, The prostate.

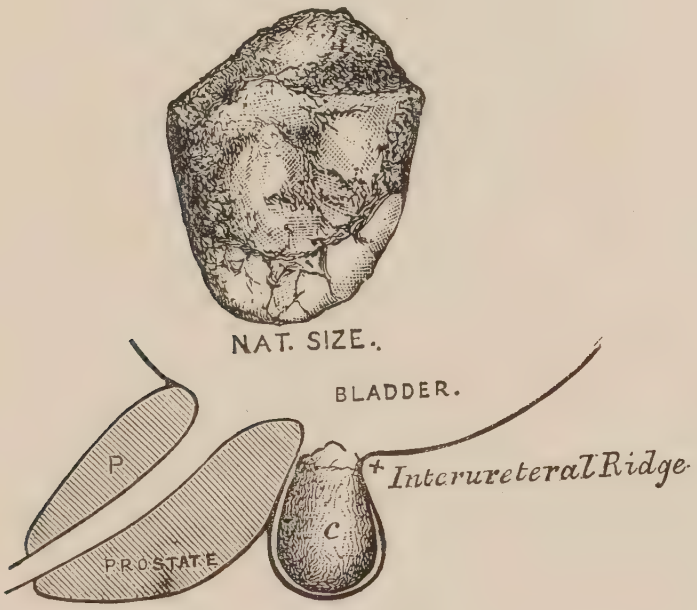
FIG. 3.—Partly diagrammatic, to show how in this case a lithotrite (S) introduced through the urethra failed to reach the calculi.

the nine calculi, which I removed nearly four months before death, but which are replaced to make the engraving complete. It will be seen what difficulties such a case must present, owing to the size and depth of the post-prostatic pouch, both to the surgeon sounding for stone and to the lithotritist. It is also conceivable that lateral perineal lithotomy would not have rid the bladder entirely of its calculous contents. There are many cases well known where experienced surgeons have left calculi in the bladder after the lateral operation, and probably such a condition as the

one described has often been the cause. Supra-pubic cystotomy here was perfectly successful; the calculus found in the pouch after death had come down from the kidney quite recently, and corresponded exactly with the other stones found in the left kidney.

Case 2.—W. O——, aged 68. Before this patient came to me he had undergone five operations of lithotrity during a period of less than two years, obtaining little, if any, relief. He was suffering greatly. I opened the bladder above the pubes, found a stone firmly fixed in a post-prostatic pouch, and took it out with a spoon exactly as one would take out half an egg from an egg-cup. The patient was entirely relieved, and made a good recovery. Fig. 4 represents the stone, which was phos-

Fig. 4.



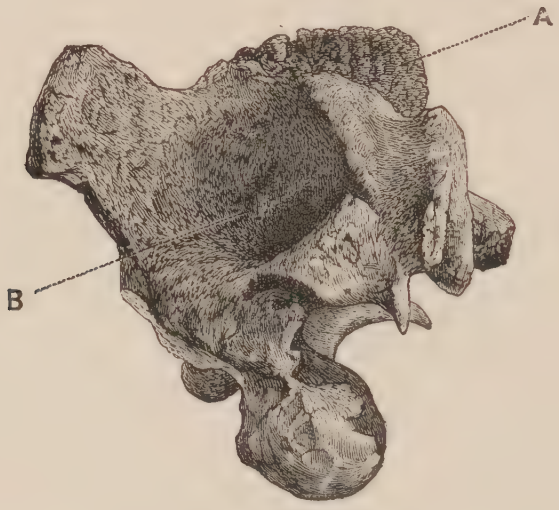
phatic, and underneath the engraving shows the relations of this calculus to the bladder and prostate. The stone presented but a small surface in the bladder, and lay in a pouch which was not deep enough to make the detection of the stone difficult by a sound introduced through the urethra, but was sufficiently deep to make successful lithotrity impossible.

The following case, however, is still more remarkable, for, while it illustrates the entire futility of lithotrity when the calculus is in a deep post-prostatic pouch, the stone is the most unusual one I have ever seen, and is really a cast of the pouch, showing all its irregularities.

Case 3.—The patient was aged 94. Some years ago he had been subjected to an operation of lithotrity, and more recently he had again undergone that operation. On both occasions the operators had been men of the greatest eminence. Since the last operation he had suffered considerably, and there were now present all the signs of calculus. The

effort of rising from his chair brought on violent vesical spasm. Driving and walking were impossible on account of the pain they caused. I told him that he had stone, and begged him to take an anæsthetic and to give me leave to remove the calculus by any means which I might find best at the time of operation, saying that his previous experience of the operation of lithotrity showed that the case was not an ordinary one, but that probably there was abnormal calculous condition present. Nothing, however, would induce him to undergo another operation, and in three months he died. Upon post-mortem examination the remarkable stone shown in Fig. 5 was removed from behind the prostate. The rough

Fig. 5.



portion marked A was all that presented in the bladder cavity, and this was evidently the surface from which the successive lithotritists had removed the more salient portions of the stone. In front of this rough surface is a smooth cup-shaped cavity, B, in which the intra-vesical prostatic projection played. It will easily be seen that it was impossible to remove this stone by any instruments introduced through the natural passages, and that it could have been thoroughly removed by supra-pubic cystotomy.

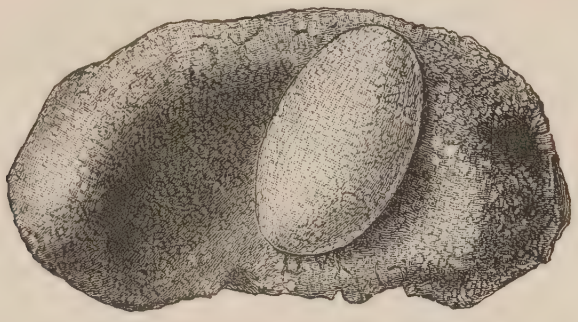
The next illustrative case is also one where the stone was of remarkable shape, being like half a bird's nest, with a loose stone like a bird's egg in it; the calculi consisted of solid uric acid, the larger one in reality presenting a cast of a not very deep post-prostatic pouch.

Case 4.—P. B——, aged 70. Seen with Dr. G. D. Thane, November, 1888. The patient had suffered from great irritation of the bladder; he was often disturbed every ten minutes to pass water; riding in a carriage caused great distress, but there was no pain after micturition; and blood had never been seen in the urine. I sounded him, found a stone, and thought that perhaps there were several stones. Three days after the patient was anæsthetised, and the stone seized with a lithotrite, but I was at once conscious that the calculus was fixed and not free in the

cavity of the bladder, and therefore at once proceeded to supra-pubic lithotomy.

When the finger entered the bladder I had reason to congratulate myself upon the decision, for there was a cup-shaped mass of uric-acid stone (Fig. 6) lying underneath a prostatic projection together with a small free

Fig. 6.



uric-acid stone. It would have been impossible thoroughly to crush and evacuate this stone by lithotrity; it was far too firmly and too deeply fixed. I removed it with a scoop, and the patient made a good recovery, and he has remained well ever since. The larger stone presents anteriorly a cup-shaped hollow, in which the intra-vesical prostatic projection lay.

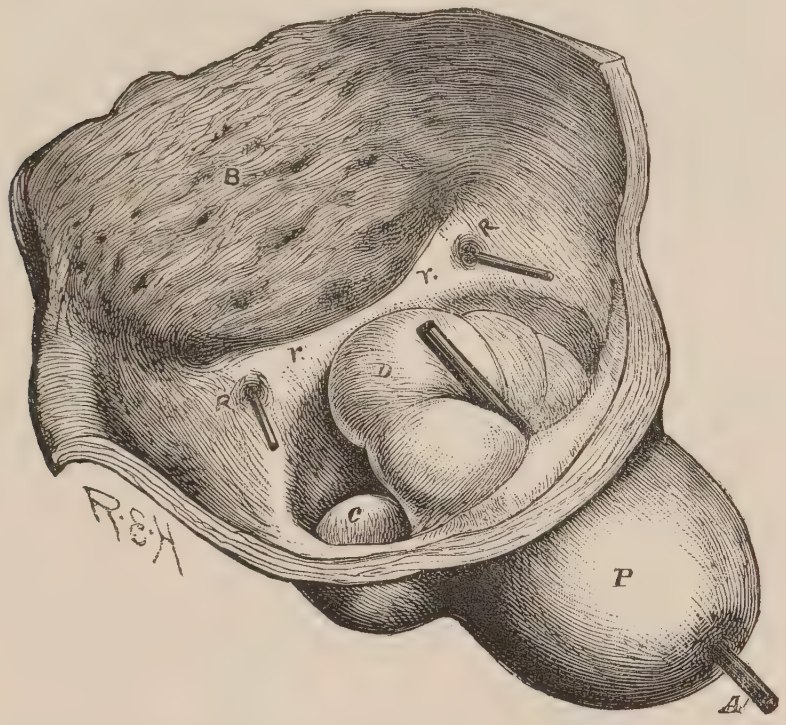
The following case is of perhaps greater professional interest than the others, for the patient had been in several hands before he came to mine, and during the time he was under my care I had occasionally to contend with the greatest difficulties due to the depth of the post-prostatic pouch; owing, however, to the fact that this condition was thoroughly realised, these difficulties were always successfully overcome. When at last the patient died, I was allowed to examine the parts concerned; and am therefore able to give a complete account of the case.

Case 5.—B. V——, aged 70, seen with Mr. H. Fraser Stokes. Retention of urine, August, 1887, and ever since all by catheter. In July, 1888, Mr. Stokes wrote asking me to see the patient, saying: "He has been sounded several times by Mr. —— and Mr. ——, and by myself, and nothing can be detected." On the 24th I visited him. The catheter was passed every three hours; there was much spasm and great pain in the penis; the urine was acid, with much pus; sp. gr. 1018. I sounded, and at first felt nothing; but after a little search found a stone, apparently on the floor of the bladder. On July 27th Mr. Moss administered ether, and I seized a medium-sized stone with reversed blades and crushed it. The operation was satisfactorily completed, and the patient made a very good recovery. The stone weighed 2 drachms 25 grains, and consisted of an oxalate centre with phosphatic incrustation. January, 1889: The patient had been very well, going six hours between his catheterisations, but he now complained of pain before and after using the catheter. He was sounded, nothing found, and the bladder washed out with a No. 16 tube and lithotrity aspirator, obtaining no result nor hearing the smallest rattle of

stone against the end of the tube. The symptoms continuing, I was asked to sound him again, and on February 5th did so, and found a stone apparently on the floor of the bladder, and withdrew the sound. Mr. Stokes, who was standing by, said he had not heard the stone, and asked if I would let him hear it. So the sound was re-introduced ; but no stone could be felt, nor was it until after careful search had been made for several minutes that the beak of the sound seemed to find its way into a pouch on the floor of the bladder, and immediately there encountered the stone. Next day lithotrity was performed. The stone was difficult to find, but was seized with reversed blades, and then carried into the cavity of the bladder. While held there we discussed the question of performing lithotrity or of opening the bladder above the pubes, and we decided in favour of the former. The stone was therefore crushed and entirely removed, a rectal bag being introduced into the bowel to press up the floor of the bladder, and to prevent as far as possible, *débris* from falling into the pocket behind the prostate. The patient made a good recovery, and continued very well and active until October, when he began to experience again a little pain after catheterism. On the 25th I sounded and found stone. The urine was very offensive, and there was much spasmodic pain before and after catheterism. Next day lithotrity was performed, and two calculi were successively picked out from the pouch on the floor of the bladder, with which I was now very familiar. Great pains were taken to evacuate every particle, and no trace of more stone in any part of the bladder could be felt ; and, although the patient's consent had been obtained to supra-pubic cystotomy, if necessary, we thought it well to be content. Unfortunately he was not at all relieved by this operation, and the spasms continued before and after each catheterism, which had to be performed every hour. This went on until November 4th, when he was so ill that I decided to open the bladder, and next morning performed the usual supra-pubic operation. Directly the finger entered the bladder it found an intra-vesical prostatic growth as large as a chestnut, growing from below and rather to the right of the vesical urethral orifice; and under the growth, at the bottom of a deep pocket, there was a calculus as large as a pea. The prostatic outgrowth offered itself temptingly for removal ; but, taking into consideration the patient's weak state, we reluctantly left it alone. He was so ill afterwards with vomiting and hæmatemesis that we were very glad we had done so. He slowly recovered, and on the 29th the wound was firmly healed. During the middle of December he had a severe attack of right renal colic, and on the 21st I washed out of the bladder a distinct calculus through a No. 15 evacuating lithotrity tube. During January, 1890, he enjoyed good health, frequently held his urine seven hours, and was entirely free from pain. On February 1st he had another severe right renal colic with rigors, and gradually passed into a dying condition, and sank on February 12th. On February 14th I made a post-mortem examination, and obtained the bladder and prostate shown in the engraving (Fig. 7). A stone has been put in to show where the stone lay during life, and it will readily be seen why difficulty was so frequently experienced in the detection of the successive calculi and in their removal through the urethra, and how it was that one calculus was never touched until the bladder was opened supra-pubically. The intra-vesical prostatic growth completely roofed over the post-prostatic pouch, and was practically in contact with the well-developed inter-ureteral ridge, so that exploring instruments were only with great difficulty, and even

uncertainty, passed into the pouch. At the post-mortem examination, the deep post-prostatic pouch was entirely free from calculus. The ureters were only slightly dilated, and the right ureter was almost blocked by a

Fig. 7.



A, Piece of bougie lying in urethra. B, Bladder-wall—post-trigonal pouch. C, The calculus. D, Intra-vesical prostatic growth. P, The prostate. R, R, The vesical openings of the ureters. *r r*, Inter-ureteral ridge.

calculus about an inch below the kidney. This was the only stone to be found. The kidneys were congested and passing into an interstitial suppurative condition. This patient evidently from time to time passed stones into his bladder from his right kidney, which were trapped by the post-prostatic pouch ; indeed, they had not a chance of escape by the urethra. It was unfortunate that his vital powers failed when they did, for there was only one more renal stone to be passed, and that was engaged in the ureter at the time of his death.

Case 6.—The stones in the case just related were only small, and it may be urged that that was one reason, at any rate, why they were not easily found, but really large stones sometimes lie in this pouch and escape detection. Fig. 8 represents a calculus weighing $13\frac{1}{2}$ drachms, which I recently removed by supra-pubic lithotomy from a patient aged 80. He lived eight weeks after the operation. The stone was hidden away in a post-prostatic pouch. An excellent surgeon had within a few months performed lithotrity in the general cavity of the bladder without, it may fairly be assumed, ever touching this large calculus.

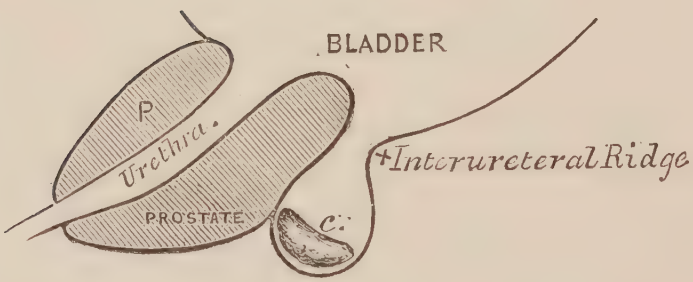
FIG. 8.

A. *B.*

The next case (Case 7) is one of a uric-acid calculus weighing 50 grains, and shown in Fig. 9, which I entirely failed to find by sounding, in the

FIG. 9.

A NAT. SIZE. B.



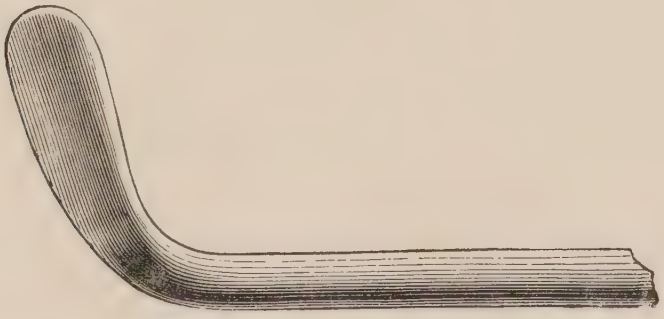
bladder of a gentleman aged 80, although I examined him three times, once when he was anæsthetised, using lithotrity tubes and aspirator and sounds of various sizes and curves. My conviction that he had a stone was, however, not shaken, and as his sufferings were severe he submitted to supra-pubic cystotomy. On entering the bladder with the finger the stone was found with great difficulty, owing to the depth of the post-prostatic pouch in which it lay. The diagram represents the parts as I found them. This case is an important illustration of what I maintain to be a fact—viz., that vesical calculus may exist under such conditions as to be absolutely out of the reach of all instruments introduced through the urethra.

If time and space permitted I could give further examples of a

more ordinary character from my practice, where I and others have failed to find stones when sounding in the usual way, owing to their lying in post-prostatic pouches. In one case the bladder was opened by lateral perineal cystotomy for drainage and exploration, and the stone was not felt by the staff or detected until the finger entered the bladder; but I think enough has been related to illustrate the importance of the post-prostatic pouch in the surgery of stone in the bladder, and to prove the two propositions with which I commenced this paper.

The practical outcome of my paper is therefore—1. The insistance in all doubtful cases of vesical suffering, where the prostate is enlarged, upon careful search being made behind that organ. In sounding in such cases it is not enough merely to reverse the beak of the sound, but thorough examination must be made, with the reversed beak, for the slit-like opening between the intra-vesical prostatic growth and the inter-ureteral ridge, which may be the sole means of access to a large post-prostatic pouch, when such a pouch exists. For this investigation a sound I have designed with a beak like a flat-bladed lithotrite will be found

Fig. 10.



Bladder sound, with broad flat beak (the author's).

useful. The broad flat beak is easily reversed when in the bladder, and it slips with greater facility than the beak of an ordinary round-ended sound under the projecting lobe of the prostate, allowing the space under it to be as fully explored as is possible by any instrument passed in by the natural passage. Help may sometimes be afforded in this exploration by introducing a rectal bag into the bowel before sounding, or by an assistant's making upward pressure with his finger in the rectum. The endoscope, as in too many of the really obscure troubles of the bladder, is here of no

use; nor is distension of the bladder with fluid, which in the case of a post-trigonal pouch is always useful, of any avail when the stone lies concealed in a post-prostatic pouch. 2. When there is reason to believe that there is a deep post-prostatic pouch it will be well, save in very exceptional cases, not to attempt lithotrity, for it is when performed under such conditions that lithotrity is likely to be discredited, because it may be impossible to clear the pouch of all stone by instruments passed in by the urethra. Case 4 is a good example of this. Here lithotrity was abandoned in favour of supra-pubic lithotomy. It is reasonable to believe that such a stone, situated as it was under a projecting prostatic mass, could not have been entirely removed by lithotrity, and the result has justified the decision, for the patient has had no return of calculous troubles since the operation, two years and a-half ago. 3. Finally, when calculous symptoms and vesical distress continue in spite of treatment, and no stone can be found by the usual methods of examination through the urethra, and also in cases where a stone is found, but lying deeply in a post-prostatic pouch, it is urged that the bladder should be opened by supra-pubic incision in preference to all others. When this is done and combined with firm upward rectal pressure, the surgeon obtains complete command over the post-prostatic pouch, such command indeed as can be obtained by no other cystotomy, while at the same time it is incomparably safer than any perineal operation, being almost bloodless, in cases which by their very nature (great prostatic enlargement) are certain to bleed freely if any incision is made from between the legs.

Surgeon-Major Keegan remarked that he considered the paper read by Mr. Buckston Browne a most valuable and important one, and that it filled a gap in the literature of stone in the bladder. On looking over the cases submitted to them this evening he was inclined to ask the question, "Is it right to subject very old men labouring under the presence of stone in sacculated bladders to any operation seeing that recurrence of stone in such cases must necessarily be very frequent?" In looking over the history of some of the cases brought forward by Mr. B. Browne, he considered that one or two of them might have been successfully dealt with by median lithotomy. He was of opinion that the sound exhibited was a valuable instrument.

Mr. Campbell Williams said that his experience, gained from Mr. Berkeley Hill's practice, concurred with that of Mr. Buckston Browne in the expediency of performing supra-pubic lithotomy instead of urethral, when the patient was the subject of enlarged prostate with a post-prostatic pouch. As Mr. March had pointed out, the supra-pubic operation was not one of great severity, being well borne by the patient. Mr. Hill had only had one fatal case. Comparing the high operation

with median cystotomy in this class of case, it was easier to perform, the former produced less shock and practically no loss of blood. It, moreover, possessed the advantage, that the interior of the bladder could be examined by the eye as well as the finger. He related the case of a gentleman, aged 66, the subject of enlarged prostate, on whom supra-pubic lithotomy was performed, and a stone weighing 355 grains removed. He made a rapid recovery, having had practically no shock. The wound was quite healed by the 16th day. The bladder in this case was examined both by finger and the electric light. After being free from all trouble for three months, the patient returned complaining of his old vesical symptoms. He was sounded and found to be calculous. Median cystotomy was performed and three small uric acid stones removed. Two had one facet each and the third had two facets. He denied having had renal colic, but close questioning evoked the fact, that, about one month before coming to me, he had a sharp pain shooting from his liver towards the groin. He then marked out with his finger the course of the right ureter. If lithotomy had been performed in this case doubtless the descent of the fresh calculi would have been attributed to inefficient evacuation. The patient suffered considerably more shock from the median than the high operation, and the wound took twenty-one days to heal as against sixteen. Another case was that of a gentleman, aged 76, on whom Mr. Hill performed supra-pubic cystotomy and removed twenty-four calculi weighing 605 grains from a post-prostatic pouch. He had a very large prostate. On recovering from ether he had copious hæmatemesis. He died on the 11th day after the operation from asthenia, seemingly due to vomiting and non-assimilation of food, although the wound was healing well. In such a case as this urethral lithotomy would have required so much time and would have been so difficult as to be impracticable. In another case, the patient, aged 64, had voided all his urine by catheter for thirteen years. The urethra was full sized until the prostatic part was reached. This admitted 10 F. with difficulty. The high operation was performed by Mr. Hill, and a stone weighing 149 grains was removed. There was no intravesical prostatic enlargement to be seen or felt—it was intra-urethral. The beak of the sound having risen over the veru montanum, entered the bladder, where it could be seen projecting from the os internum, laying flat upon the trigone. In this case urethral lithotomy must have caused very severe bruising, and as the patient could not void his urine, the constant passage of an instrument would have been most painful. In this case the bladder was healed on the 17th day. The average time of healing in Mr. Hill's cases was sixteen days.

April 20th, 1891.

ON KOCH'S TREATMENT IN TUBERCULOSIS OF THE LUNG AND IN LUPUS VULGARIS.

By G. A. Heron, M.D., F.R.C.P. Lond.

I desire, before touching upon the subject of our proposed debate to-night, to express my thanks to the Council of the Society for the honour they have done me in inviting me to open this discussion. I hope and believe some good will come out of it, in the shape of a further clearing up of the position which Koch's remedy for tuberculosis is to occupy in clinical medicine. To the clear defining of that position nothing, it seems to me, will give so much help as a statement of what is seen to accompany and follow the use of this remedy in the human victim of tuberculosis. That being my belief, it is clearly incumbent upon me to state to the Society what I have observed in my own practice in hospital cases, and to travel as little as possible outside of my own experience in this matter. So much has been already stated in the medical press, both at home and abroad, concerning the uses of tuberculin, that were I now to attempt even to summarise and to comment upon these records of a new subject, I should have to occupy much more than the whole of the time allotted to-night to our weekly meeting. What has been published concerning Koch's new remedy is, as a matter of course, familiar to all medical men who take interest in the progress of medicine. This branch of our discussion to-night should, I apprehend, come, for the most part, from those amongst us who have, from various causes, devoted themselves more to the study of the literature of the subject than to its investigation at the bedside.

The patients under my care at the City of London Hospital for Diseases of the Chest were all subjected to the same rules, so far as was possible, both before and during the time that they underwent Koch's treatment. Many of the patients asked to have this treatment followed out in their own cases, and in no instance was it commenced unless the patient had previously expressed a wish

to have it, and had spent at least one day, and, as a rule, two or three days, in a ward where several other persons were undergoing the treatment. The tuberculin used for these patients was sent to me fortnightly by Dr. Libbertz, of Berlin, and it was, therefore, practically impossible that any error of consequence could have arisen in the important matter of the trustworthiness of the drug. The remedy was administered by means of Koch's syringe in the usual way—that is to say, it was injected under the skin between the shoulder blades and the line of the spines of the dorsal vertebræ, and with those antiseptic precautions which Koch himself practised. The doses ranged between 1 mgrm. and 1000 mgrm. in amount, and in cases of tuberculosis of the lungs were given, as nearly as possible, every day. Before the treatment of any case of suspected tuberculosis of the lung was begun, the expectoration of the patient concerned was searched daily for tubercular bacilli. No proof of the presence of the bacillus of tubercle was found, so far at least as the evidence of Ehrlich's test goes, in four of the cases of tuberculosis of the lung, and one of them had no sputum.

Following another of Koch's recommendations, the temperature of each patient about to undergo this special treatment was taken and recorded for two or three days before the first dose of the remedy was given. As the treatment of the case went on, this early record of what had been the patient's temperature when it was uninfluenced by tuberculin, was found to be very useful for purposes of reference and comparison.

Choice of Cases for Treatment.

In my selection of cases for the purpose of ascertaining by their means the value of tuberculin as a remedial agent, I was influenced solely by the very explicit statement of his own views on that important matter, which Koch himself made to me for my guidance. In effect he told me that, so far as tubercular affection of the lungs is concerned, he hoped for the best results from his treatment only in cases in an early stage of that disease. By this, he explained to me, he meant cases where a small portion of one lung only was found to be affected to a slight extent, and where there was no evidence of excavation. That was the type of case in which Koch looked for cure of the disease. Where the tubercular condition was found to affect slightly, in the way already indi-

cated, small portions of both lungs, there, too, a cure was reasonably to be expected. Any departure from these types of cases in the direction of increase of the extent of the disease lessened, by so much, in Koch's opinion, the chances of a cure, although even in more advanced cases amelioration of symptoms was, he believed, likely to result from the use of the remedy. It required, however, but a very brief experience to convince me that the sort of case which, in my opinion, expressed in its symptoms of disease what Koch meant by his description of cases likely to be cured, was a kind of case by no means common amongst the people who came to the City of London Hospital for treatment. And especially as regards the male patients was it remarked that, not only among the in-patients, but also among the out-patients, it was very uncommon to find examples of these very early stages of tuberculosis of the lungs. This fact was also observed by several of my colleagues in charge of the out-patient department, and who have to deal there with a practice which is considerably larger than that of any other similar hospital in London or elsewhere. Among the female patients one was now and then found illustrating perfectly, so far as I could judge, what Koch had described to me as the typical case for the use of tuberculin. In the dearth of such cases, it was often necessary to select, for this special treatment, those patients whose conditions of disease were least distant from those of the typical case. Thirty-seven patients were treated by means of tuberculin under my supervision in hospital. It is, of course, practically impossible for me to bring all those cases here to-night. I propose, therefore, to speak in general terms of them all.

To the 37 patients with whose cases we have to deal (some of them are still under treatment), over 1200 injections had, up to April 7th, been given. The smallest number of injections given to any patient for purposes of treatment was 8; the largest number was, up to April 7th, 66. The shortest residence in hospital of any patient who was treated there was 21 days, and 3 cases of lupus remain to-day under treatment, 2 of whom came into the hospital at the end of November, and 1 in the first week of December.

To the pain produced by the prick of the needle of the syringe all the patients soon became more or less indifferent. The minimum of pain from this cause is secured by using only smooth

and sharp-pointed needles, and by completing the puncture quickly. In most cases the local pain of the puncture soon passes away, but often among the first injections it begins to return in from three to twelve hours or even later, and then it will be found to be among the early symptoms of the beginning of the reaction which is likely to follow the injection. This pain, when it sets in with the other symptoms of reaction, is sometimes very considerable in degree, and it may persist until the rise of temperature due to the reaction has passed away. In one case, which was an exceptional one, there were, in addition to this pain, redness and swelling extending not only from the site of puncture, but also from the little swelling which showed where the tuberculin had been expelled from the needle into the tissues. This swelling was hard throughout its extent, and very sensitive when even gently handled. In this patient these symptoms usually set in about eight or twelve hours after the injection had been given, and in thirty-six hours they had, for the most part, disappeared, leaving behind them only a slight sensitiveness to gentle pressure, and a little redness over the site of the swelling. These exceptional symptoms did not begin to show themselves until after the patient concerned had received four injections. After the tenth injection they became so trivial in amount that they were not noted, and they only once again showed themselves in a marked manner during the treatment of the case. This patient was an ill-fed-looking, undergrown girl of 16 years of age, extremely susceptible to the action of the drug. Tubercular disease involved the upper part of both of her lungs, to about the level of the fourth rib on each side, and the glands on both sides of her neck. On the left side of her neck was an open sore, from which, before treatment was begun, there flowed a considerable discharge of pus. The sore was about the size of a florin—rather smaller.

In none of the thirty-seven cases, nor in any of the cases, was there ever, as a consequence of the treatment, any evidence of abscess. In several of these patients, after about forty injections had been given, the skin, which had been so often pierced by the needle, became somewhat indurated. This was shown by the increased resistance in certain spots to the passage of the needle through the skin, and by a feeling of toughness of the skin, when it was gently pinched between finger and thumb. This condition of skin was apt to be accompanied by an increase of sensitiveness

to the prick of the needle. As a consequence of this in eight patients, five men and three women, the condition of whose backs was more or less troublesome, the injections were given below the skin over the deltoid muscle, and on the outside of the forearm just below the elbow-joint. In every instance, within two or three hours, the new site of puncture became red and swollen, and very tender when touched. Redness and swelling also appeared about the same time over the spot where the fluid had left the needle and entered the tissues. This was all the more remarkable, because no such symptoms had previously occurred in any of those eight patients while the remedy was administered between their shoulder blades. It would seem to indicate that a local tolerance of the drug had gradually been established at the earlier site of the injection while small doses of tuberculin were being given, which was wanting in the arm when, on first puncturing it, a large dose of the remedy was injected.

Some of the Points noted during Reaction.

Of the common features of the familiar reaction which often follows the administration of tuberculin, I do not propose at present to speak, because they are already too well known to need further notice now. There are, however, certain points on which I may be allowed to dwell for a short time. From November 23rd, when tuberculin was first used at the City of London Hospital, until a month after that time, the temperatures of the patients undergoing the treatment were taken, as nearly as was convenient, every two hours during the day and night. As a rule, the thermometer was placed under the tongue in the usual way. The temperatures were taken with great care by Dr. Adamson and Dr. Scott, who for more than three months acted as my house physicians in connexion with this special work, and of whose devotion to their duties I cannot speak too highly. It was very soon evident, from observations made in the wards, that temperatures taken in the mouth are more liable to error than those taken in the axilla or in the rectum. This was found to be especially the case when the temperatures were taken during the night. The thermometers when placed under the patients' tongues were occasionally found to register too low a temperature. It was noticed that patients who had been sleeping with their mouths open, or who had, as sometimes happened, drunk a little cold

milk or water just before using the thermometer, returned temperatures of between 95° and 96° F., when their temperature in the axilla was ascertained to be a point or two above 97° or below 98°, and in the rectum below 99°. Dr. Adamson, who is a healthy man 25 years of age, found that the temperature under his tongue was only 95° in the morning after his cold bath, and that the thermometer remained persistently at that point until he sat down to breakfast, about a quarter of an hour after he had bathed. After that meal, the same thermometer placed under his tongue rose to the ordinary temperature of the body. These low temperatures were taken or checked by instruments tested at Kew. Of course the patients to whose temperatures reference is here made were, as a rule, more or less under the influence of tuberculin; and many of our observations show that temperatures of from 96·6° up to 97° are not very rare in such cases, especially when the temperature has reached its lowest point after a reaction. These low temperatures generally occurred between midnight and 4 A.M., and were not noticed to bring with them symptoms of any kind. What has just been said was well illustrated in the case of a girl of 14 years of age, referred to in 'The Lancet' and 'British Medical Journal' under the initials H. D.

Another case, also mentioned by those journals, is that of B. W——, a man aged 22 years, whose temperature, instead of rising, habitually began to fall in five or seven hours after the injection of tuberculin, and often reached as low a point as 96° in from nine to twelve hours after the injection. In this case twenty-seven injections of tuberculin were given in six weeks, in doses beginning with 2 mgrm., and ending with 100 mgrm., which was the amount given in each of his last ten doses. On three occasions only did this patient's temperature rise above 99°; once it rose to 99·8° after a dose of 5 mgrm. of the remedy, and on two occasions well-marked reactions followed the injection of 100 mgrm.

Another patient, F. S——, a man 22 years of age, received fifteen injections in fifteen days, in doses ranging from 2 mgrm. to 20 mgrm. He never showed any symptom clearly caused by the remedy, for his temperature only once reached as high as 99·8°, and on only three other occasions it touched 99·6°, a point to which, however, it had risen before he began to use tuberculin. On two occasions he had very trivial headache, but it could not be certainly ascribed to the use of the remedy. He left the hospital because the treatment did not affect him as he saw it affect others.

These two cases were instances of tuberculosis of the lungs, and in both of them tubercular bacilli were found repeatedly, and in

large numbers, in the sputa. F. S—— is an undoubted illustration of the fact, that certain cases of well-marked tuberculosis of the lung do not react in an unmistakable way to tuberculin in moderate doses. In addition to the fact that F. S—— had numerous tubercular bacilli in his sputum all the time that he was in the hospital, the physical signs of his disease were plainly marked, and indicated, surely, tubercular infiltration of about one-third of the left lung in its upper part; and one or two patches of crepitation in the same lung towards its base were due, in all probability, to the same cause.

Very severe reactions with high temperatures are, as a rule, consequences of large doses of tuberculin given at the beginning of treatment. Where treatment is commenced with doses of 1 or 2 mgrm. it is exceptional, in my experience, to find that there result either very high temperatures, or other symptoms of very severe reaction. Therefore I have seen, in my own practice, very little indeed of those symptoms of apparently serious depression which have often followed the administration of tuberculin. In only one of my patients have any of these symptoms been present. That was an instance of lupus in a young girl 21 years of age, whose case is referred to in 'The Lancet' and 'British Medical Journal' under the initials L. F. Hers was the first case treated by means of tuberculin in this country. Following the Berlin practice, I began her treatment with a dose of 10 mgrm. The reaction which followed was moderately severe, and it was only on the sixth day after the giving of the injection that the temperature again touched the normal line. Twenty-four hours after the administration of the tuberculin, the temperature in the rectum was 104°. On the ninth day of treatment a second injection was given. The dose (10 mgrm.) was taken from the same specimen of tuberculin which had been used for the first injection. Eight hours afterwards the temperature in the rectum was 105·8°. That high temperature had been preceded by three severe rigors, great nausea, and delirium, besides the ordinary symptoms of a severe reaction. I had seen in Berlin several reactions to tuberculin more marked by nervous symptoms than was the case here. This experience, however, decided me never again to begin the use of tuberculin by the administration of a large dose of it, and so 5 mgrm. of the drug have since then marked the limit of my largest first dose, for, as occurred in this instance, and as indeed

often happens, much more violent effects are apt to follow the second dose of the remedy than have resulted from a first dose of it of the same strength. I have been guided in increasing the dose in all cases, more by the presence of such symptoms as rigor, nausea, vomiting, headache, and marked joint pains, than by a rise of temperature unaccompanied, to a moderate extent, by any of these symptoms. The dose should never be increased until tolerance of the dose in use is shown to be established by the absence of these subjective symptoms, and then the dose can be increased by 10, 20, 50, or even, towards the end of treatment, by 100 mgrm. daily. With proper attention to these points I am strongly of opinion that tuberculin can be administered with safety.

There was in this case of L. F—— another condition which was often seen amongst my cases in a modified form, but only in her case were its characteristics markedly exaggerated, especially on three occasions. This condition has been noticed by others, and its chief features are marked increase of the rate of respiration, with a striking softness of pulse and of radial arteries. The pulse rate was not at these times materially quickened in this case, nor was it irregular. It varied then between 72 and 96, while the respiration numbered from 60 to 96. There was no dyspnœa, but merely extreme rapidity of breathing, which recalled to my mind what we have all noticed in dogs on a hot summer's day, when they sit panting in the sunshine, with the exception that, of course, the patient's tongue was not protruded. If a dog in this condition have his attention arrested by any cause, he ceases for the time to breathe quickly. So it was with my patient, because when she was spoken to while in this condition, the rapid breathing ceased at once, and she conversed without effort of any kind and in her usual tone of voice. It seemed as if she breathed in this way because it was a comfort for her to do so. She has not shown any symptom during the five months that she has spent in hospital which, in my opinion, could be called hysterical. The symptoms referred to above occurred, as a rule, while the temperature was between 101° and 102°, and falling after a reaction and after all other subjective symptoms had passed off. These special features first showed themselves prominently thirty-four hours after the administration of the second dose of tuberculin, then the pulse was 96, and the respirations numbered 60 per

minute. On another occasion the pulse was 96 and the respiration 90, and on a third occasion the record stands: pulse 72, respiration 96. These symptoms lasted from half an hour to less than an hour at a time. The instances just quoted are the most marked ones, but putting pulse-rate first and that of respiration second, 50 and 30, 60 and 24, 130 and 60, 120 and 40, were rates often noticed in the case of L. F. These conditions were unaccompanied by any other symptoms.

Increase and Extension of certain Physical Signs during the Use of Tuberculin.

In many of the cases there was noticed increase of the mucous râles after the use of the remedy, when their then condition was compared with what it had been before treatment was commenced. The increase was expressed in this, that the moist sounds became certainly coarser, and there seemed to be more of them at the site of the disease. Also, in parts of the lung where certainly no evidence of the existence of disease had been observed before tuberculin was used, there were noticed thereafter, in many instances, crepitations which were undistinguishable from those usually heard over the site of early tuberculosis of the lung. This was observed in several instances in those patients who were suffering from tuberculosis affecting apparently one lung only, and in them it was often observed about the apex of the lung which had seemed healthy. These features were noticeable after a reaction had set in, and they continued probably for twenty-four or thirty-six hours. It was hardly possible to be sure as to the period of their duration, because as soon as the temperature fell to the normal limit, another injection of tuberculin was given, and then these physical signs usually repeated themselves. In most of these instances the new signs were noticed to disappear as the treatment progressed, but in exceptional instances they certainly remained for one or two weeks after the beginning of treatment; though even in these cases the newly developed sounds, it was noticed, always rapidly diminished in amount and in extent. In no case did these new physical signs exist when the patient left the hospital. I was able to satisfy myself that these new physical signs were sometimes, but not always, accompanied with increase of dulness of percussion. These conditions of lung may be reasonably explained by comparing what probably takes place

there, with what we see going on in and around patches of lupus vulgaris. The marked swelling of the affected parts, and of apparently healthy tissue for some distance from them, which forms so familiar a characteristic of the effect of tuberculin on this skin disease, may very well have its counterpart in the tubercular lung, and this would account, it seems to me, for all these new physical signs, although it is difficult to understand why evident dulness on percussion should not always form one of them. In parts distant from the first recognised lesion of the lung, and having no continuity of tissue with it—as, for instance, the apex of the opposite lung,—it may very well be the case that the existence in them of centres of disease, not detectable by the stethoscope, explains the reason why, when tuberculin acts, new patches of crepitation are developed there. It is only reasonable to believe that in every case of tuberculosis of the lung there are, especially in the beginning of the disease, affected portions of tissue which do not convey the evidence of their condition to our senses.

Skin Eruptions.

The patient L. F——, already mentioned, was the only one under my care who showed that peculiar skin eruption which Dr. Radcliffe Crocker has so well described in the 'Lancet' of November 22nd, 1890. She had it on two occasions early in her course of treatment. There was nothing in these attacks to distinguish them from the many others like them, that have been often mentioned in connection with the use of tuberculin. Another patient, A. H——, a woman aged 28, had tuberculosis of about one-half of each of her lungs, with cavity at the apex of the right lung. After she had been twenty-nine days under treatment, and had received twenty-two injections of the remedy, there appeared a purpuric rash, which affected in patches her thighs and legs from the groins to the ankles. It began with small groups of papules, which I noticed in one or two instances were at first colourless, but gradually became red or reddish-brown in colour. Between the groups—they did not loose their colour on pressure—was apparently healthy skin, and at first the little groups of papules varied in size from that of a pin's head to that of a farthing. At the same time that this eruption first appeared, there was slight tenderness and swelling in the right wrist, and at the outer and back part of the right elbow there was a little puffiness, with

bluish discoloration. Between the radius and ulna, about half-way down the right arm at its posterior aspect, there was distinct swelling, but without tenderness. There was also pain in both knee joints, and in the legs, and in the left elbow. In four days both ankles were swollen, and there was swelling of the right fore-arm in front, in its upper third. The right arm at the level of the swelling measured half an inch more than the left one. There was no evidence of abnormality in the liver or the spleen. On the sixth day these swellings disappeared, and the rash, which had meanwhile become confluent all over the lower limbs, began to fade. On the seventh day after the appearance of the rash there was slight puffiness over the forehead, extending from the line of the middle of the left to the outside of the right orbit, and from above the eye-brows to the roots of the hair, and measuring $4\frac{1}{2}$ inches by 2 inches. The puffiness had more the appearance than the feeling of a swelling, and was without discoloration of skin. This swelling disappeared in two days, and did not return. The hæmorrhagic rash, which had seemed to fade for a short time, recurred, and covered practically the whole of the lower limbs. In this condition the patient, at her own wish, returned to her home. She wrote to me on April 8th, saying: "Occasionally the rash reappears, and when it does I am unable to walk about for a day or two, but that has only happened twice during the six weeks I have been at home." On one occasion only was albumen detected in this patient's urine, and then there was but a mere trace of it. Her urine as a rule amounted to between 35 ounces and 50 ounces, and presented no feature of special interest. It was examined three or four times a week. This woman received no injection after the purpuric rash appeared. Her highest dose of tuberculin was 8 mgrm., and she had eight marked reactions in response to the twenty-two doses given to her.

Condition of Urine.

It may be convenient to state at this point, that only in eight of these thirty-seven cases was albumen found in the urine, and its presence was always temporary. No observation of consequence was made when the urine was examined under the microscope.

Other Complications.

Of these there were but three of any importance. A girl, whose

case is referred to in the journals under the initials H. D——, whose age was 14 years, received on the twenty-first day of treatment a dose of 8 mgrm. of the remedy, and to it there was no symptom of reaction. On the day following the girl's temperature began to rise, and no reason for this could be found. It was a rule in the wards that when a patient who was using tuberculin showed any important symptom which could not be readily accounted for, then, at once, the use of the drug was stopped. To that rule there was never an exception. Accordingly, the treatment was discontinued whenever this unexplained rise of temperature was noted. This girl developed pleurisy of the left side, with a moderate amount of effusion; and it ran an ordinary course. In a fortnight the treatment was resumed and continued until thirty-six injections had been given, the last three of these having been doses of 100 mgrm. each. The girl had abundant tubercular bacilli in her sputa, and both of her lungs were tubercular, the infection extending, without excavation of lung tissue, on the left side to about the level of the fourth rib in front and the sixth behind, and on the right side to about the level of the second rib in front, and behind to about the line of the spine of the scapula. This patient did very well; and I hope there has been an opportunity for you to examine her to-night. Another complication occurred in the case of a girl (S. M——), 18 years of age, who had coarse râles over the upper half of her left lung, in front and behind, and a slighter affection of her right one. She, after having been two weeks under treatment, and on the day after she had received her ninth injection, developed a pneumothorax of the left side. She blamed the treatment for this condition, and in accordance with her own wish she was not again submitted to the injections. In a few days she passed under the care of my colleague, Dr. Eustace Smith, whose patient she had previously been, and made a good recovery so far as the pneumothorax is concerned. With regard to the cases of laryngeal tuberculosis, they are too few in number amongst these patients to justify a decided opinion being formed about them in connection with the use of tuberculin. It is, however, a fact that not one of these cases did well as regards the throat condition; indeed one of them, the man referred to in the table under the initials A. D., aged 27, became rapidly worse while under that treatment. Mention is made in the table of a prominent part that was played by the appearance of a hectic condition in certain cases.

Of course patients with tuberculosis of the lungs frequently undergo exacerbations of temperature. Dr. Chaplin, Resident Medical Officer at Victoria Park Hospital, kindly looked up for me one hundred ordinary cases of phthisis, which had not been treated by Koch's method. He found that during a residence of six weeks in hospital eight of the hundred had exacerbations of temperature. It will be noticed that, with one exception, all my patients who showed hectic symptoms had done so before treatment was commenced. So far as this experience goes, it merely shows that tuberculin did not cure the hectic state of these patients. In all the cases where it occurred, in these patients, the exacerbation of temperature, after lasting for a varying number of days, gradually passed off. But, undoubtedly, it showed its usual tendency to recur, in spite of tuberculin, whether given in large or small doses. In the case of one patient, A. H——, we had her blood examined by Ehrlich's test while she was undergoing an attack of marked hectic, and at the height of her purpuric eruption, but no tubercular bacilli were observed, although very careful search was made for them.

Explanation of Tables.

In the table submitted for your inspection I have divided my patients into three classes. In the first class are placed those who come within the limits, as I understand them, of Koch's category of those consumptives in whom cure might reasonably have been hoped to follow the use of the remedy. I cannot say that any one of my patients has been cured beyond danger of relapse. Nor did Koch himself regard such cases, even when the best results had been obtained from treatment, as safe from relapse. He emphasises that opinion of his very strongly in his well-known paper of November 13th of last year. In my second class of cases—those designated as being in the second stage—I include patients whose conditions of lung disease place them outside of that class of people who are suffering from tuberculosis of the lung in its commencement. The physical signs of their disease, and their general symptoms, showed that without any evidence of cavity being detectable they had considerable tracts of lung tissue involved in the tubercular process. (Figs. 1 and 2.) In my third class are those who had evidence of cavity in their lungs. In every patient

of all three classes, judging from general symptoms and from physical signs, the tubercular process was not tending towards cure, nor was it stationary before treatment was commenced. The contrary of these conditions obtained in every instance.

Fig. 1.

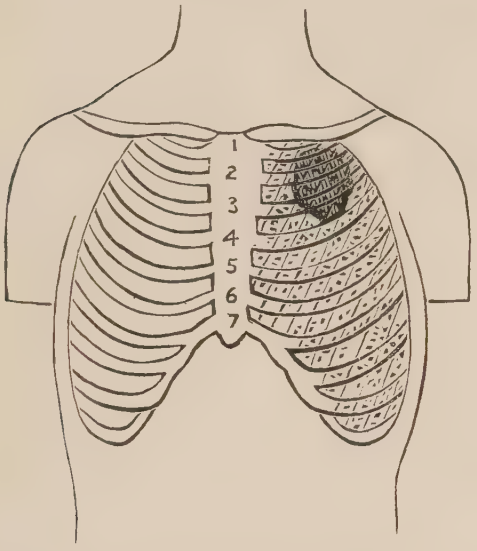
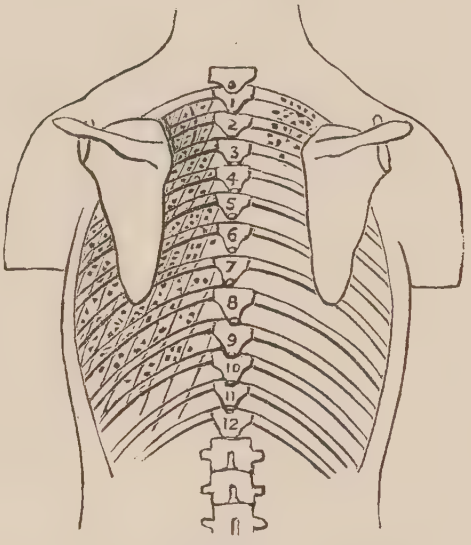


Fig. 2.



Figs. 1 and 2 graphically represent the condition of G. M——, sixth case on p. 388, after a course of treatment, on April 7th.

Evidences of Improvement.

The table indicates that a certain number of these patients have improved in health—some more, some less—under the use of tuberculin. (Figs. 3, 4, 5, and 6.) This improvement was best manifested

Fig. 3.

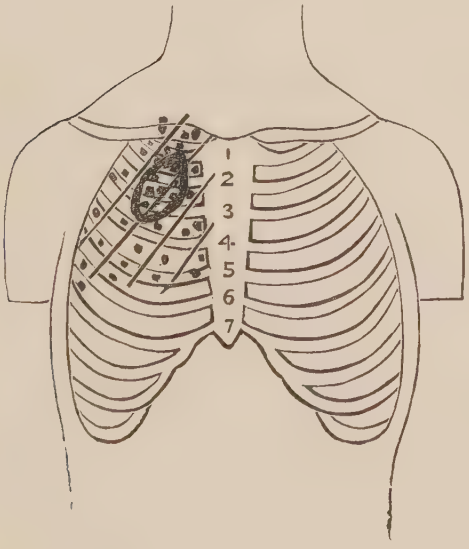


Fig. 4.

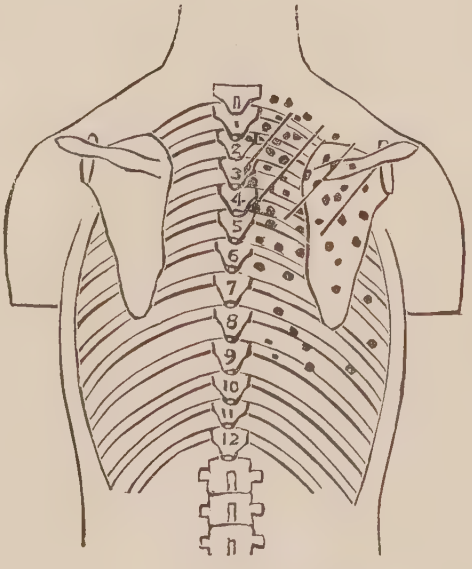


Fig. 5.

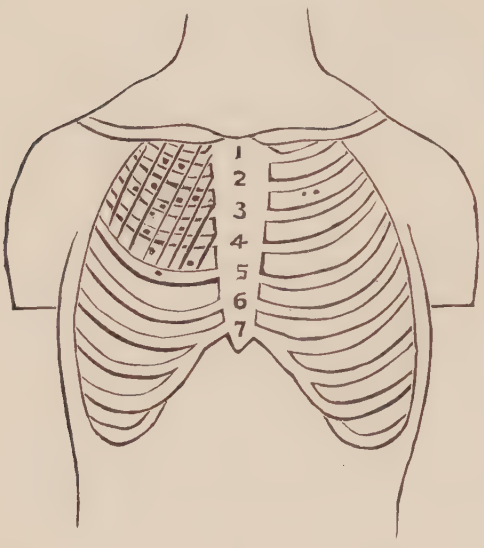
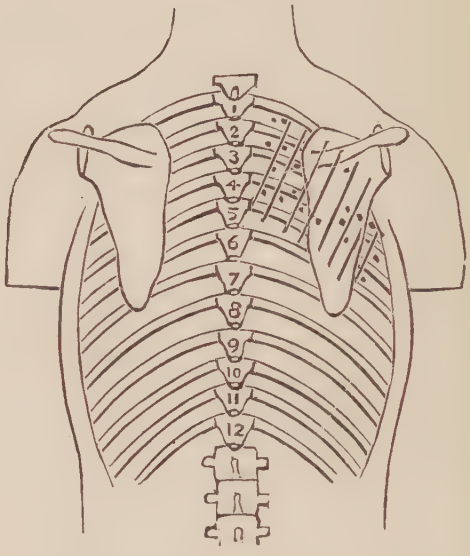


Fig. 6.



Figs. 3 and 4 represent the condition of A. D——, aged 40, the last case on p. 389, at the beginning of the treatment, and Figs. 5 and 6 the condition of the same patient on April 7th.

by a gradual disappearance of troublesome cough and night sweats, and by a material, and sometimes very marked, increase in the patient's sense of freedom of breathing power. It was remarkable how several of these patients themselves observed this indication of improvement in their condition, and drew attention to the fact that they could walk about the hospital grounds for an hour or so, and then go up to their wards without once stopping to rest by the way because of their shortness of breath, and without being subject to fits of severe cough after the exertion of coming upstairs. Before treatment these patients had been unable to walk even upstairs to the wards without stopping several times on the way to cough and rest.

Then, as regards the evidences of improvement which had their expression in the physical signs of these patients. Weight, as a rule, was gained. This, however, is a condition common to most consumptives during their sojourn in hospital, whether they are or are not treated by tuberculin. Evidences of improvement in the lung itself were found in a change in moist sounds heard there before treatment was commenced. This change consists in a gradual lessening of those qualities of a râle, which enable the trained ear to distinguish between moist crepitations and the dry cracklings associated with sounds due to friction. The moist

Patients' initials.	Age.	Sex.	Condition of disease.	Tubercular bacilli.	Sputum decreased. From to	No. of injections.	Highest dose in mgrms.	No. of reactions.	Complications.	Weight. Gained.	Weight. Lost.	Duration of treatment. Days.	Results.	Remarks.
F. S. ...	22	M.	1st stage	Very many	2 dr. ... scanty	15	20	None ...	None ...	2 lb.	...	16	Great improvement	Eleven weeks after treatment improvement maintained. At work 7 weeks as packer. Tubercle bacilli, many.
C. H. ...	27	M.	1st stage	None	½–1 oz. unaltered	20	30	10, all below 10 mgrm. dose.	None ...	Unaltered	Unaltered	35	Improvement.	Sputa examined again and again; no tubercular bacilli found.
F J. W.	29	M.	1st stage	Many	1 oz. ... scanty	36	100	6 below 10 mgrm. dose; 4 above	Hectic and abdominal pain; sore over xiphoid	Unaltered	Unaltered	70	Unimproved	Has not been seen since treatment ended.
Z. ...	31	M.	1st stage	Many	1–3 oz. abt. 2 oz.	44	350	6 below 10 mgrm. dose; 6 above	None ...	1½ lb.	...	66	Improved materially	——
J. K. ...	35	M.	1st stage	None	½ oz. ... scanty	55	1000	13	None ...	...	3 lb.	67	Decided improvement	Treatment just ended.
T. H. ...	26	M.	1st stage	Few to many	1–4 oz. unaltd.	18	20	5 below 10 mgrm. dose; 1 above	Hectic before, during, and after treatment	...	7 lb.	32	Became worse	Hectic before, during, and after treatment. Had attacks of diarrhœa with pain on four days.
T. P. ..	24	M	1st stage	Very many	1 oz. ... ½ oz.	40	400	3 below 10 mgrm. dose; 2 above	Laryngeal tuberculosis	10 lb.	...	66	Improved in lung; larynx trifling improvement	——
H. D....	14	F.	1st stage	Very many	½–1 oz. .. scanty	36	100	3, all up to 10 mgrm. dose	Pleurisy; old hip-joint disease	12 lb.	...	42	Great improvement	——
H. B. ...	28	F.	1st stage	Many	1½ oz. ... none	32	100	4 up to 10 mgrm. dose	None ...	10 lb.	...	46	Very great improvement.	Excellent health 2 months after treatment. Is at work as a general servant.

Cases of Tuberculosis of the Lung—continued.

Patients' initials.	Age.	Sex.	Condition of disease.	Tubercular bacilli.	Sputum decreased.	No. of injections.	Highest doses in mgrms.	No. of reactions.	Complications.	Weight.		Duration of treatment.	Results.	Remarks.
										Gained.	Lost.			
					From to							Days.		
M. de W.	28	F.	1st stage	Many	½ oz. ... 1 dr.	31	50	10 up to 10 mgrm. dose; 2 above	Tonsillitis; trivial attack	3½ lb.	...	42	Improvement	—
O. H. ...	21	F.	1st stage	Few	Scanty ... scanty	69	300	9 up to 10 mgrm. dose; 9 above	None ...	6½ lb.	...	126	Great improvement	Still in hospital.
L. B. ...	23	F.	1st stage	Very many	½ oz. ... scanty	38	100	4 up to 10 mgrm. dose; 9 above	Hectic before treatment	6 lb.	...	56	Improved materially	Sputum after treatment ceased all viscid mucus. Hectic before treatment.
E. S. ...	20	F.	1st stage	Very many	2 oz. ... 1½ dr.	24	100	3 up to 10 mgrm. dose; 4 above	Hectic before treatment	Unaltered	Unaltered	52	Improvement. Still in hospital	Hectic before treatment begun. Trace of albumen on five occasions, four of them before treatment. Had rheumatic fever 12 months ago.
J. D. ...	20	F.	1st stage	None	None	10	4	7	None ...	Unaltered	Unaltered	30	Unimproved. Left hospital at her own request	In the hospital 2 years ago with history of hæmoptysis. No sputum then.
G. M.*	23	M.	2nd stage	Very many	1 dr. ... scanty	38	100	3 up to 10 mgrm. dose; 1 above	Hectic after treatment	...	2½ lb.	41	Improvement	Sixth evening after treatment patient became hectic, remaining so 8 days. For 26 days thereafter temperature normal.
B. W ...	22	M.	2nd stage	Very many	Unaltered; (1 oz. ... 2 oz.)	27	100	2 up to 10 mgrm. dose; 2 above	Laryngeal tuberculosis	2 lb.	...	30	Improvement	Laryngeal condition unimproved.

* See Figs. 1 and 2.

Cases of Tuberculosis of the Lung—continued.

Patients' initials.	Age.	Sex.	Condition of disease.	Tubercular bacilli.	Sputum decreased.	No. of injections.	Highest dose in mgrms.	No. of reactions.	Complications.	Weight.		Duration of treatment.	Results.	Remarks.
					From to					Gained.	Lost.	Days.		
E. G. ...	36	M.	2nd stage	Very many	2 oz. unaltered	47	500	8 up to 10 mgrm. dose: 3 above	Syphilis ...	...	...	69	Improved materially	Hectic before treatment. After seventh injection treatment stopped, there having been no reaction up to 12 mgrms. Iodide of potassium given for 10 days. Reactions followed.
R. N. ...	36	M.	2nd stage	Few; none found after injection	½ oz. unaltered; mucus only; purulent before	38	100	12 up to 10 mgrm. dose; 4 after	None ...	5 lb.	...	52	Unaltered	——
J. G. ...	29	M.	2nd stage	Many	4 oz. ... 1½ oz.	33	100	16	None ...	2½ lb.	...	51	Improved slightly	Is able to return to work as museum attendant.
L. S. ...	45	M.	2nd stage	Many	2½ oz. ... 1 oz.	34	100	4 up to 10 mgrm. dose; 1 above	None ...	3½ lb.	...	43	Improved materially	——
T. L. ...	49	M.	2nd stage	Many	2½ oz. ... 1¼ oz.	22	100	2 up to 10 mgrm. dose; 5 above	Twisted arteries	10½ lb.	...	35	Improved	Twisted arteries.
A. A. ...	16	F.	2nd stage	Many	½ dr. ... none	40	20	21 up to 10 mgrm. dose; 1 above	Tuberculosis, glands of neck; sore left side	...	4 lb.	84	Improved. Sore healed	Lungs both affected. Physical signs unaltered.
A. ...	...	...	...	...	... ...	...	...	...	...	...	...	...	...	Still under treatment.
K. ...	...	...	...	...	... ...	...	...	...	...	...	...	...	...	Still under treatment.
A. D.*	40	M.	Cavity	Many	1 oz. ... scanty	44	100	3 up to 10 mgrm. dose; 2 above	Hectic before treatment	8 lb.	...	52	Great improvement. Signs of cavity gone	Slight hectic before treatment. At work at the docks as a stevedore. Temperature now normal (April 7th).

* See Figs. 3, 4, 5, and 6.

Cases of Tuberculosis of the Lung—continued.

Patients' initials.	Age.	Sex.	Condition of disease.	Tubercular bacilli.	Sputum decreased.	No. of injections.	Highest dose in mgrms.	No. of reactions.	Complications.	Weight.		Duration of treatment.	Results.	Remarks.
										Gained. Unaltered	Lost. Unaltered	Days.		
A. D. ...	27	M.	Cavity	Very many	From to 1 oz. ... 6 oz.; increased	33	70	1 up to 10 mgrm. dose; 1 above	Laryngeal tuberculosis; hectic			56	Became worse	Ulceration of vocal cords, with irregular granulations developed during treatment. Anæmic epiglottis. Arytenoid cartilages and both cords hyperæmic before treatment. Marked hectic from beginning to end.
A. W....	23	M.	Cavity	Many	4 oz. ... 1¼ oz.	40	40	11 up to 10 mgrm. dose; 9 above	Hectic from first	...	1 lb.	62	Trivial improvement	Hectic from the first.
C. M. ...	23	M.	Cavity	Very many	5 oz. ... ¼ dr.	8	3	1	Hectic before treatment	Unaltered	Unaltered	9	Improved considerably	Improvement not due to treatment probably. Had marked hectic (102·4°) before treatment. Much less marked since.
S. M. ...	19	F.	Cavity	None	1 dr. ... 4 dr.; increased	41	150	14 up to 10 mgrm. dose; 3 above	Hectic before treatment	7½ lb.	...	79	Slight improvement	Slight hectic before treatment. None for a week after treatment. Occasional trace of albumen.
S. M. ...	18	F.	Cavity	Very many	2 dr. ... 2 oz.; increased	9	6	4	Pneumothorax, left lung	Unaltered	Unaltered	14	Became worse	Pneumothorax occurred 4 days after patient discontinued treatment at her own request.
A. H. ...	28	F	Cavity	Very many	1 oz. ... 1 dr.	22	8	12	Purpura	...	5 lb.	35	Became worse	Trace of albumen on four occasions during rash.
								ANÆMIA.						
A. B. ...	18	F.	Anæmia	...	...	15	30	7 up to 10 mgrm. dose	None	8 lb.	...	28	Very great improvement	Injected with her consent, as a control case at first, afterwards because of unexpected reaction.

Cases of Tuberculosis—continued.

LUPUS VULGARIS.

Patients' initials.	Age.	Sex.	Condition of disease.	Tubercular bacilli.	Sputum decreased.	No. of injections.	Highest dose in mgrms.	No. of reactions.	Complications.	Weight.		Duration of treatment.	Results.	Remarks.
										Gained.	Lost.	Days.		
A. N....	16	F.	Face; elbow.	...	...	50	1000	18	...	Unaltered.	Unaltered.	104	Arm very greatly improved, face much improved	Beyond very slight headache on two or three occasions, this patient had no subjective symptoms of general reaction.
E. F. ...	57	F.	Face; hand.	...	...	50	400	8	...	10 lb.	...	124	Much improved Relapse	During last weeks of treatment fresh centres of infected skin noted on right temple and on nose.
E. R. ...	27	F.	Patch under chin; face.	...	...	59	1000	9	...	10 lb.	...	128	Much improved	—
L. F. ...	21	F.	Face, nose, and lips.	...	...	59	1000	6	...	18½ lb.	...	135	Great improvement; relapse	Signs of relapse during last month of treatment; fresh spots below right nostril, at root of nose, and on right cheek.
J. B. ...	27	F.	Right side of nose; right cheek.	...	. .	35	1000	6	...	...	...	63	Very good	Slight case.

N.B.—When this paper was read before the Medical Society, these tables were to have been shown in the form presented here. It was, however, found that the table as it is given in these pages would have been unwieldy on the walls of the Society's room; hence certain additions to the tables are now published.—G. A. Heron.

sounds are heard to approach in quality nearer and nearer to the dry ones, and to become, at least to my ear, indistinguishable from them. In patients who do very well under the treatment there comes a great diminution of both moist and dry sounds; for in a month or six weeks of treatment with tuberculin it is often to be noticed in these cases that, during ordinary respiration, no râles, dry or moist, are audible, although when the patient coughs there come little showers of crepitations, which, however, are only to be heard during the first two or three inspirations that follow the cough. In not one of my patients have these signs absolutely disappeared, though in four of them they have all but gone. In order to ascertain the exact value of these different degrees of improvement, much more time and prolonged observation of these cases are necessary before, in my opinion, we can gauge with accuracy, and with justice to all concerned, the real worth of tuberculin as a healer of tuberculosis of the lung.

Probable Usefulness of Tuberculin.

In cases at the very commencement of that disease, and when it is not seemingly quiescent, I think the remedy should be tried, because no drug is known to have anything like the beneficial effect of tuberculin upon those tubercular diseases which can be seen and handled, and evidence, it seems to me, is accumulating tending to show that the beneficial effects which we see produced in lupus and other similar conditions by the use of tuberculin, are also produced in the diseased lung which we cannot see. And another reason for its use in very early stages of lung disease is that, in cases of this kind, there is, so far as I know, no evidence to show that there is material risk in employing tuberculin. The remedy has been already used in several of these early cases, and no evil, but, on the contrary, material good to the patients concerned has resulted in no inconsiderable number of these early instances of lung disease. In such cases as these I can see no reason why there should be any hesitation in using tuberculin as a remedial agent. In lupus vulgaris, however, the beneficial effects following the proper use of tuberculin are so evident and so marked, that I feel sure this remedy will very soon become an essential part of the treatment of that disease. In two of my cases of lupus fresh points of infection were distinctly evident, and these manifested themselves to me, and to the patients and others, during the last four weeks or

so of the five months' course of treatment to which these patients were submitted, and while they were taking daily increasing doses of tuberculin, ranging between 200 and 1000 mgrm. The cases now referred to are those marked on the table as L. F. and E. F. In the former case the points of fresh infection were observed upon the upper lip and right cheek, and in the latter over the right temple. These two cases both show, as is indeed already well known from other sources, that in spite of the excellents results which, up to a certain point, practically always follow the use of tuberculin in lupus vulgaris, danger of the reappearance of the disease is so great that, as Koch himself says, we must make "use of all other auxiliary methods to assist the action of the remedy."

From a paper of this kind it is inevitable that much that is of importance and of interest must be omitted. I have tried to indicate certain points in this question concerning the value of tuberculin as a remedy, but of necessity I have left very much unsaid.

I congratulate the Society upon the fact that you, Sir, preside over our debates, for there is no one into whose hands the conduct of our proceedings could be better confided, or in whose sense of what is just and right to all concerned we could more surely place a perfect trust.

Dr. Theodore Williams considered that the author had taken a very moderate view of the value of this treatment. He himself had not had so many cases under his care. Of those which had been treated at the Brompton Hospital at present no report had been published, and he would only refer to his own cases. He went over to Berlin to study the treatment, and he came back with the impression that he had not seen many material successes; only a small number appeared to be genuinely improved. He had had fourteen undoubted cases of phthisis under this treatment; all were under treatment for a considerable time, none less than six weeks, and others as long as ten. For the most part they were injected three times a week, in quantities varying from a milligramme to a decigramme. In five cases the disease was in what Dr. Heron would describe as the first stage; one was a case of softening, five had single cavities, and three had double cavities. Only six out of these fourteen improved generally in weight and strength during the injections; the rest were all decidedly worse. As to the effect on the lungs in the five cases of the first stage, four of them showed cavity formation somewhat rapidly, tubercle bacilli became more numerous, and lung tissue was constantly found in the sputum. In the excavation cases also extension of the disease was observed, and in one of the double cavity cases two other cavities formed. At the end of the treatment excavation was found in twelve of the cases, excavation accompanied by tubercular spread in six, and tubercular spread without excavation in one. In two of the cases the cavities appeared to contract, and the patients left the hospital better, but the others could not be said to have done at all well;

in fact, they could not compare with the results of the ordinary expectant treatment. The principal effect of the injections was to convert tubercular masses into cavities, and to set free tubercle bacilli, which led to fresh lung infection. Many of these patients, when placed, at a later date, on the ordinary tonic treatment of the disease improved considerably. Dr. Williams had understood that when this treatment was combined with residence at high altitudes, the results were more favourable, but his experience did not accord with this, as he had seen several patients, who would undoubtedly have improved at high altitudes, lose ground under the combination of the two treatments. He remarked on the extraordinary effect the drug appeared to exercise in increasing the amount of lung tissue expectorated. It was found in the sputum in almost all the cases, and was detected as long as the patient remained under treatment. He had observed when at Berlin that at most of the hospitals they did not appear to look for lung tissue. In most of the cases there was also an increase in the number of bacilli expectorated. As regards diagnosis, it might prove very useful, but he observed that in two cases of laryngeal tuberculosis, though there was general, there was no local reaction. The searching nature of this remedy might be made use of by combining other drugs with it, and so eventually attacking the bacillus in its own nest after it had been tracked thither by the tuberculin.

Dr. Radcliffe Crocker desired to direct his remarks to the treatment of lupus by this drug. He quoted a case which showed the advantage of giving the injection locally, for reaction was obtained after the ordinary injection in the back had ceased to have effect. In one case, in a boy, who left the hospital with scarcely any perceptible lupus, he hoped he had obtained a cure, for all that remained were some little brown nodules below the level of the skin. After three weeks at the seaside, however, the nodules had appeared above the skin, and showed signs of incrustation. As regarded permanence, therefore, this remedy did not compare favourably with scraping alone. But in the treatment of lupus he had for some time not only scraped, but had swabbed the surface with sulphuric, or carbolic, or some other strong acid, and by this means he had found the time before recurrence considerably lengthened. The great drawback to Koch's treatment was that it seemed to stop short just where one hoped it would go on. He had recently adopted a modification. He scraped away as much as he could and then injected, and the result so far in one case was very good. He thought we could only look on it as a supplementary treatment which would not replace other methods. He had treated a marked case of lupus erythematosus by injections, and the patient, a woman, had great general and but little local reaction, and there was only slight improvement in her condition.

Dr. Bagshawe (St. Leonards) remarked that the power of Koch's remedy over tissue infiltrated with tubercle was well proved. The question was whether in a given case, this would be exerted for good or for evil? Would it be exerted simply in eliminating diseased tissue, or would serious results occur elsewhere in the process of elimination? He instanced the case of a gentleman, aged 32, the subject of tubercular pneumonia, in whom a consolidated patch had been left in the middle lobe of the left lung near the angle of the scapula, and in whose sputa bacilli were found. In consultation with Dr. Powell it was decided to use "tuberculin," ten small injections were given in three weeks with the usual results. During the next three weeks the lungs remained quiet. Then (February) the consolidation broke down into a cavity, with pro-

fuse expectoration, and fever. These subsided in a week, but a septic condition arose, with evidences of ulceration in the bowels, and a round ulcer under the tongue. There was hæmaturia probably due to an ulcer in the pelvis of the left kidney, where pain was felt. At the present time (20th April) all evidences of secondary ulceration have subsided. There is no longer diarrhœa or tympanitis, no albuminuria, and the sublingual ulcer has healed. The cavity in the lung has contracted. Cavernous sounds have subsided, and percussion is no longer wooden. The sputa are not two drams in twenty-fours and no bacilli are found. The temperature remains high (101—102°) and there is some extension of dulness in front of the lung. In spite of this local and general improvement the patient's future remains very uncertain.

Dr. Acland wished to ask Dr. Heron which of his long series of cases had been under observation in hospital before the injections of Koch's fluid were commenced. He thought that it was of great importance in these enquiries to eliminate the effect of the good food, warmth, and even temperature on persons whose ordinary conditions of life are so different from the comforts of a well managed hospital. He thought this source of fallacy should be eliminated. Dr. Acland asked for information as to whether it had been found that the best results from "tuberculin" were obtained by producing tolerance of the drug by frequent injections or whether the best results followed decided reactions such as were observed when the drug was administered at less frequent intervals. Dr. Acland concluded by expressing his surprise that now after so many months the "Medical Society" should be discussing what was practically a secret remedy; he thought that the methods adopted for the introduction and sale of the drug were alike harmful to the best interests of science and medicine.

Dr. Heron, in reply, said that the patients not only expressed themselves generally improved, but there was evidence of it in the condition of the lung; the coarse sounds got finer and the moist sounds drier; that, generally stated, was the condition of the lungs of the first class of cases, and it applied also to the others. In his experience he had not found in any instance satisfactory evidence that a new cavity had formed in any part of a lung. There was only one case in which this remained doubtful. He thought that Dr. Williams' experience of tuberculin as a cavity-maker was an exceptional one. He had often found a considerable amount of lung tissue as well as bacilli, but not to such an extent as Dr. Williams had described. He could not demonstrate in any instance extension of the disease in the lungs while under treatment. He had found the injection in the back to be quite as satisfactory as local injection in lupus. It was remarkable that, though the patient might remain quiescent at first, a marked reaction might take place after a fourth or fifth injection, and this might be the herald of a beneficial result. In lupus he had obtained the best results with those patients who reacted well at first with the drug, and who soon ceased to react at all. Often where there was no general reaction, a local reaction was observable after the injections, and this was a valuable indication. He said that, as a rule, his cases had not been treated at first in hospital by ordinary means, but they came in direct from the out-patients' department. Referring to the question of ethics, he said it was quite enough for him to know that a man like Dr. Koch wished to keep a certain part of his work to himself. He felt sure that this was not done for a selfish motive, but for the benefit of mankind at large.

April 27th, 1891.

A CASE OF ECTOPIA VISCERUM.

By Thomas Elliott, M.D. (Tunbridge Wells).

On the morning of March 26th, 1891, I was summoned to see a newly-born infant, as the midwife could not make out what was the matter with the child's navel—"it had a tumour on it, and she had never seen such a thing before." I went at once, and found that the mother had been attended by a midwife, had had a favourable labour, and had given birth to a child half an hour prior to my arrival.

On examining the infant, I found that it was a male child, apparently born at full term, and the mother subsequently informing me that she last menstruated about nine months ago; that it began on June 8th, 1890, and lasted for a week or longer, and that she was never out more than a few days in her calculations. Conception, therefore, most likely took place on June 20th, 1890, as pregnancy terminated 280 days afterwards, on March 26th, 1891, the day the child was born. The infant was also well developed, and perfect in every respect, except in the region of the abdomen. Here the stomach, small intestines, and colon (ascending, transverse, and descending), with the distended bladder and both testicles, were lying outside of the abdominal parietes, having completely protruded through an opening, about 2 inches in length, at and below the umbilicus. He cried lustily, and, when the bladder was pressed upon, ejected urine. The intestines were full of meconium, which could be forced out by pressure viâ ani. The intestines also were of a port-wine colour, and were covered with a villus-like growth, which much resembled nævi.

Having asked my friend, Mr. G. S. Watson, M.R.C.S., to see the case, we agreed that no operation could benefit the child, as it was utterly impossible to replace the various organs within the abdomen, and, even if we did succeed in this, it would be impossible to cover them or retain them there. It was surprising to find, by passing a finger into the abdomen, how little space there was within. By passing the intestines over to the left side, the

edge of the liver and the gall-bladder could be seen. From stethoscopic examination the heart and lungs were found to be normal.

The infant lived $26\frac{1}{2}$ hours, and was named by his father "Nonsuch"—a not inappropriate name, I think. The child weighed 6 lbs. 6 ounces, and his height was 18 inches.

Other measurements taken were: chest, 11 inches; round head, 13 inches; length of leg, $6\frac{1}{4}$ inches; of foot, $2\frac{3}{4}$ inches; and of arm, $7\frac{3}{4}$ inches. The mother, whose age was 38, had born seven children before, five boys and two girls. Of these a boy and a girl had died at the ages of 4 and $1\frac{1}{2}$ years. None of the children suffered from deformities, except the eldest son, whose left ear presented a funnel-shaped appearance, and looked as if it had been folded downwards, and held in that position by a band of skin. His hearing, however, was perfect. The mother did not attribute the condition of her last-born to any so-called maternal impressions. She had a couple of frights during her pregnancy; one when she was two months gone, through one of her children being run over by a light spring cart, but the child was not hurt; the other was when she was seven months gone, through another child having a leg broken. But these things did not really frighten her—but of course, some anxiety was caused. When quickening took place she felt very ill and queer for a day or two. For four years, I ascertained, she had suffered from a pendulous belly, and, on placing my hand on the abdomen, I found that the abdominal parietes were very thin, and the enlarged uterus stood prominently forward. She also suffered from varices of lower extremities and on abdomen. The accompanying photograph (cabinet size) gives a very faithful representation; and the specimen, which I have shown you, has been presented to the Museum of the Royal College of Surgeons of England; and Mr. J. H. Targett, F.R.C.S., has kindly furnished me with the accompanying anatomical report.

Report of a case of Ectopia Viscerum.

A male infant, born at full term, who lived only $26\frac{1}{2}$ hours. The head, chest, and limbs were all normal. In the abdominal wall there was a large opening, situated to the right of the attachment of the umbilical cord. It measured $4\frac{3}{4}$ inches in circumference. Through it the intestines and certain other viscera protruded, and were found lying upon the exterior of the abdominal wall. The opening was bounded above and on the right side by a thin edge of abdominal wall, in which the peritoneum was continuous with the skin; on the left side there was the flat-

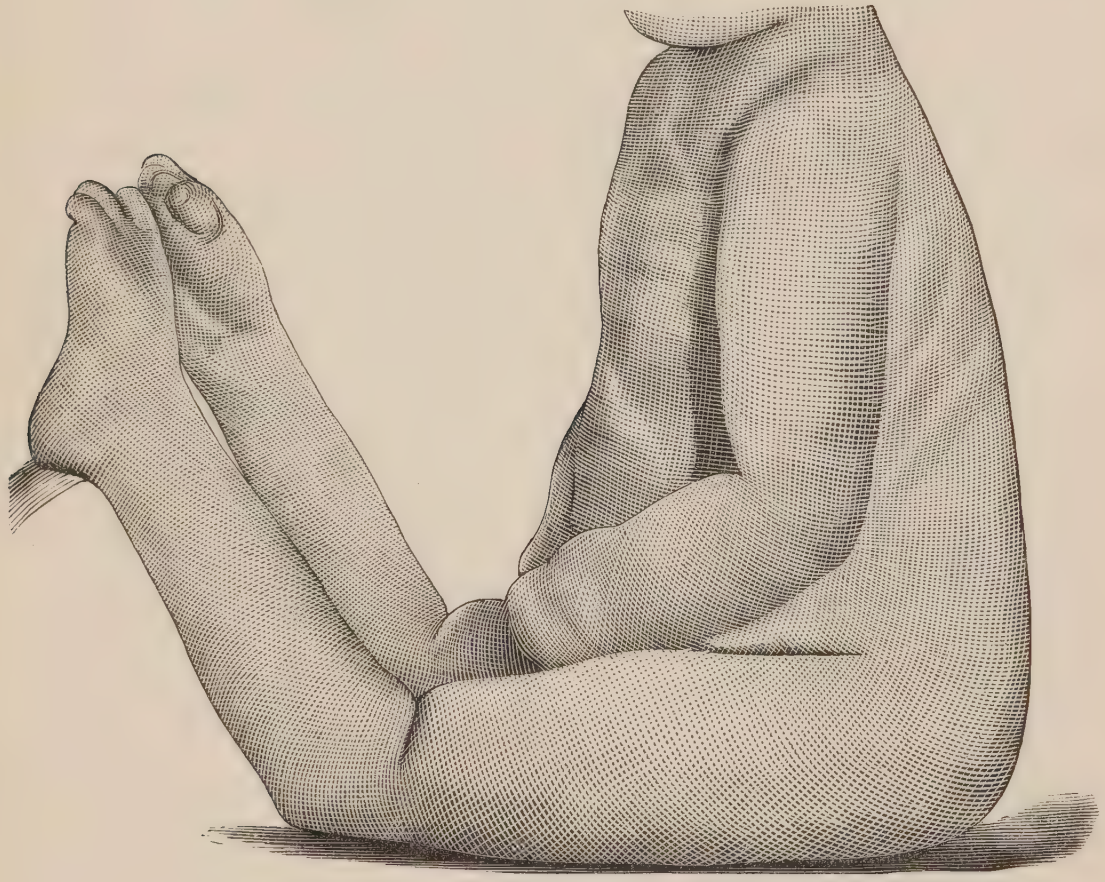
tened attachment of the umbilical cord, which was triangular in shape, and of which the round ligament of the liver, and the right hypogastric artery formed respectively the upper and lower borders; while below the opening the skin joined the peritoneum over the fundus of the bladder which here formed a prominent swelling. The organs which had protruded through the opening and were lying outside, included the lower three-quarters of the stomach, the whole of the small intestine, and colon, to within three inches of the anus, the testes and mesorchia, and a part of the bladder. The mesorchia measured half an inch in length and were attached on either side of the colon, just above the level of the pelvic cavity. The bladder was much elongated, owing to its upper end having been protruded from the abdomen. It measured from the apex to the veru montanum $2\frac{1}{4}$ inches, and the part which protruded was $\frac{3}{4}$ inch in length. The urachus came off normally and passed by the left to the umbilical cord. Owing to its displacement the right hypogastric artery crossed in front of the bladder to reach the cord. The gubernacula testinum extended down the inguinal canals to the external rings, but there were no peritoneal pouches in the canals, the testes being entirely retained as mentioned above. All the coils of the intestine were matted together by recent lymph, and the remaining viscera which protruded were similarly covered. Those within the abdominal cavity were unaffected. The liver, spleen, adrenals, and thoracic viscera were normal, but the ureters and pelves of the kidneys were dilated to a moderate degree. The opening in the abdominal wall was apparently due to want of closure at that spot, rather than to any form of rupture.

CASE OF GENU RECURVATUM ON BOTH SIDES.

By Edmund Owen, F.R.C.S.

Mr. Edmund Owen showed an Irish girl of $3\frac{3}{4}$ years who had been the subject of congenital genu recurvatum. The mother had had typhus fever seven or eight months before the confinement, when she had imagined herself to be a fish. It was improbable that this active imagination had any influence in causing the physical defect. The child had evidently been badly packed in utero, for though it was a "head first" presentation, the feet emerged resting upon the shoulders. On admission to the Great Ormond-street Hospital the child's favourite attitude was sitting, with the back of each foot resting upon the collar-bone, the knees being hinged with the salient angle backwards—like a stork's. The heads of the tibiæ were displaced so far forwards upon the trochlear surfaces, that the back of the femoral condyles and the intercodylar notch could be clearly defined in the popliteal space. There was a transverse furrow at the front of the knees, in the retiring angle, close above which the patellæ could be made out. Flexion of the knees in the proper direction was absolutely im-

possible; even when the child was under the influence of chloroform the legs could not be brought into a line with the thighs. On February 11th Mr. Owen divided the quadriceps extensor on each side by a free transverse incision, which fully exposed the trochlear surfaces. The wounds were closed with continuous



sutures and dressed with sublimate gauze, the limbs being fixed straight in a box splint. The incisions healed promptly. The knees could now be flexed to a right angle, and the child was acquiring some power over them, Mr. Sydney Jones, the Registrar, noting on April 21st that she could easily stand on the right leg. Massage and manipulation were still being employed. Mr. Owen remarked upon the comparative rarity of the deformity. The instances which he had previously met with had been in infants; he had never seen so marked a case as this, nor one which could not be remedied by treatment less severe than that of myotomy. This operation had been preferred to tenotomy of the ligamentum patellæ, as possibly promising a better immediate result and permanent improvement.

Mr. Adams congratulated Mr. Owen on the excellent result he had obtained, though he himself would have preferred to have done the operation subcutaneously.

REPORT OF 104 CASES OF ENTIRE EXCISION OF THE TONGUE FOR CANCER.

By Walter Whitehead, F.R.C.S., F.R.S. Edin.

During the last twenty years my operations upon the tongue for cancer amount to 139 cases. I find that out of these 139 cases of which I have a record there have been 119 recoveries and 20 deaths, which gives a mortality of 14·3 per cent. As the main object of my communication, however, is to place before the profession an accurate and reliable record of my personal experience in excision of the entire tongue with scissors, I shall discard, in the first place, ten of the successful cases, as they were operated upon with the galvanic écraseur. I shall also exclude twenty-five successful partial excisions, for although the excisions were performed with scissors, I am anxious to adopt a definite standard by which the merits of the operation may be duly estimated, and more easily contrasted with the results of other methods of operating for the same object. Hitherto it has been the custom to place the results of total and partial excision in parallel columns, ignoring the palpable fact that the risk and difficulties in the two instances are certainly not identical. Where the disease has advanced beyond the tissues of the tongue the operation is also much more severe, and attended with much greater danger. By this arrangement I reduce the number of my total excisions to 104, with a mortality of 20, or a percentage of 19·21, against 14·3 in the gross number of my cases. In estimating these results it has been thought desirable to draw a clear distinction between cases in which the tongue alone was excised, those in which the tongue was removed together with the glands, and those in which division or removal of portions of the jaw was an additional part of the operation. Unquestionably the risks and dangers of these different operations are not the same, and it is misleading and unjust to classify them together. Taking my own cases as an example, in excision of the tongue alone the death rate is only

4·5 per cent., compared with 77 and 57 per cent. respectively when glands and jaw have been involved.

I cannot better illustrate the misleading influence of grouping together all these different and distinct operations than by referring to the statistics published by Mr. Butlin in his work on "Diseases of the Tongue." Mr. Butlin stated, which is perfectly true, that I supplied him with my statistics of tongue excisions in 1884, and they represent forty-eight cases with nine deaths, but although the entire tongue was excised in every case, there was nothing to show or explain how much more than the tongue was removed. On referring to these cases, I find that in only two was the operation confined to excision of the tongue alone; in every other case it was complicated with either the removal of glands, portions of the jaw, or some other addition to simple excision. To show more clearly how valueless were statistics compiled on this basis, Mr. Butlin contrasted the results of my excisions with those of Mr. Baker, who classifies amongst his returns cases in which a portion only of the tongue had been removed. If we may be guided by the tables I produce, it may be roughly estimated that the mortality from the simple excision of the tongue amounts to only a point over 4 per cent.; for they clearly show that sixty-six patients may lose their tongues with a possible loss of three. Now I consider that I am entitled to draw marked attention to the particulars of these three patients who died. In the first place, the youngest was 56 and the oldest 70, and the average age of the three, 64. One was a man with advanced phthisis, who died on the second day from the rupture of a pulmonary abscess; another died on the twelfth day from syncope, after the floor of the mouth had completely healed; and the third was a woman 70 years of age, who died on the fourteenth day from inanition. When we take into consideration the ages and causes of death in these cases, I think that these tables very effectually show what a slight risk there is in excising the tongue when the operation is not complicated by the removal of contiguous structures.

Before I proceed further I wish to acknowledge the debt of gratitude I owe to my colleague, Mr. Alexander Wilson, for collecting and arranging my statistics. In order to present my cases in as accurate and trustworthy a form as possible I handed to Mr. Wilson all my notes and memoranda. He communicated with every patient whose address could be traced, and with the

representatives, when it was possible to find them, of those patients who had not survived. I am also indebted to Mr. Wilson for the several tables which he has compiled with infinite patience and industry. I think I ought to state, in justice to myself, that throughout my practice I have operated upon cases indiscriminately, and that I have never allowed the extent of the disease or the emaciated condition of the patient to deter me from operating when I have seen any reasonable prospect of prolonging life or giving even the slightest prospect of temporary relief to suffering. Had I selected my cases, and operated only in the more simple and hopeful, to the exclusion of others, I venture to believe that the mortality might have easily been reduced to a very much smaller percentage. I do not know that I have much to amend in the description I gave in 1881 of the operation for excising the tongue with scissors ; but I will first of all briefly recapitulate the essential features of the operation, and later I will enter a little more fully into some of the important details.

1. The patient should be completely under the influence of the anæsthetic during the first stage of the operation, but afterwards only partial insensibility should be maintained. 2. The mouth should be securely gagged, and kept fully open throughout the operation. 3. The head should be supported in such a position that, whilst the best light is secured, the blood tends to gravitate out of the mouth rather than backwards into the pharynx. 4. A firm ligature should be passed through the tip of the tongue for the purpose of traction. 5. The first step in the operation consists in dividing the reflection of mucous membrane between the tongue and the jaw and the anterior pillars of the fauces. 6. Rapid separation of the anterior portion of the tongue from the floor of the mouth. 7. Securing, if possible, the lingual arteries with Spencer Wells' forceps prior to division. 8. Passing a ligature through the glosso-epiglottidean fold before finally separating the tongue. 9. The application of a mercurial solution to the floor of the mouth, followed by painting the surface with an iodoform styptic varnish.

When once it has been decided to remove a patient's tongue, the sooner it can be done the better ; at the same time it may be occasionally judicious, especially with those who are in a state of feeble health, to sacrifice a few days for the purpose of generous feeding, and more especially to obtain a cleanly condition of the

mouth. With such patients digestion is often deranged, the bowels loaded, the skin inactive, the teeth foul and coated, all influences which have a tendency to lessen the chances of recovery. For an anæsthetic I recommend the use of chloroform, although I know of no objection to ether, which I think has an advantage in feeble subjects. The important point is to obtain deep anæsthesia before commencing the operation. The effectual gagging of the mouth is a point of great importance, for, on the one hand, it is necessary to keep the mouth wide open, whereas on the other we must avoid so pressing back the jaw as to embarrass the respiration. In fact the aim in gagging the mouth should be to place the jaw, as it were, in the position it occupies in semi-luxation forwards. As much depends upon the position of the patient during the operation, I hope I may be excused if I dwell rather fully upon a detail which I have found by experience to afford the greatest convenience to the operator. In the first place, the patient's head must be neither too high nor too low; the mouth ought to be, roughly speaking, on a level with the surgeon's axilla. The head should be firmly held erect with a slight inclination forwards, in such a position that the light will fall well into the mouth. It is, of course, always desirable to take advantage of the best obtainable light. Great inconvenience is generally caused by the persistent tendency of the patient to slip down on the operating table, and the best means to prevent this is for the patient to recline on an almost perpendicular back-rest, with the thighs bent at a right angle over the elevated central portion of an ordinary operating table. In private practice any joiner will readily improvise a substitute for this arrangement. A sand-bag makes a very convenient support for the head. The ligature passed through the anterior portion of the tongue is a great aid throughout the operation, and much depends upon the dexterity of the assistant in anticipating the intentions of the operator, and in always making traction in exactly the right direction, his aim being to make tense those tissues which are immediately to be divided.

The first step in the actual operation consists in the separation of the tongue from its attachment to the floor of the mouth and the anterior pillars of the fauces, and I would lay stress upon the way in which this is done, because the ease with which the operation is continued depends largely upon the freedom with which this separation is carried out. The two structures principally

responsible for the retention of the tongue within the mouth are the frænum and the anterior pillars of the fauces, and if these are completely divided in the first instance, the tongue may be so freely drawn from the mouth that the operation is practically converted into an extra-oral excision. Extended practice has made me conduct this part of the operation with less deliberation and more rapidity than was my habit in my earlier cases. Instead of the cautious snipping I originally advocated, I now boldly cut until I get close to the vicinity of the main arteries, disregarding all bleeding, unless an artery distinctly spurts, when I twist it and proceed. The more profuse the general oozing the more rapidly I proceed, my object being to get as quickly as possible to the main arteries, as I have confidence that all subsidiary bleeding will cease immediately after their division. There is, in reality, no difficulty in determining the actual position of the lingual arteries, as they are practically invariably found in the same situation, and it requires very little experience to seize them with a pair of forceps before dividing them. If this be done, there need not be the slightest hæmorrhage from this source. When once the vessels are effectually twisted, the rest of the tongue may be removed without any further anxiety about hæmorrhage; but it is desirable before finally severing the last attachments, to pass a loop of silk through the glosso-epiglottidean fold, as a provisional measure of security, in case it may become necessary to make traction on the posterior floor of the mouth either to assist respiration or to arrest any possible consecutive hæmorrhage. Traction on this ligature of itself arrests hæmorrhage, and makes it an easy matter to secure any bleeding vessel. As the retention of this ligature is a source of some annoyance to the patient, I always remove it at the end of twenty-four hours. As a matter of fact, I cannot recollect its having been required in more than two cases, and but for the certain sense of confidence it gives to those who are left in charge of the patient I should dispense with the precaution altogether, were I influenced solely by the amount of benefit I have derived from its use.

The treatment of the floor of the mouth after the tongue has been removed is a matter of considerable moment. The first object is to make the cut tissues as far as possible aseptic, and for this purpose I am still in the habit of swabbing the parts with a mercurial solution, and, after drying, finally painting the surface

with the iodoform styptic varnish which I introduced in 1881. This preparation, in addition to its antiseptic properties, has the advantage of lessening the discomfort which follows when the surface is left unprotected, and it also enables the patient to take food in the ordinary manner almost immediately after the operation. The mercurial solution I prefer is that of the biniodide, and the strength I am in the habit of using is 1 in 1000. I have recently made, and I think with advantage, a slight addition to the iodoform varnish. My original custom was simply to substitute for the spirit ordinarily used in the preparation of Friar's balsam a saturated ethereal solution of iodoform, but now I prefer to mix with the ether one volume in ten of turpentine. This addition has a very marked influence in promptly checking the capillary oozing, which occasionally prevents a dry surface being quickly secured.

I was at one time in favour of suspending all alimentation by the mouth for the first four days, and feeding the patient entirely by nutritive enemata; but for some years I have entirely abandoned this practice, and I now feed the patient with liquids by the mouth as freely and as early as possible, only using enemata when it is necessary to supplement the amount of food the patient is otherwise able to take; and I find that if a coat of the varnish is applied daily, patients rarely have any difficulty in taking an adequate amount of sustenance. It fortunately happens that the patients appreciate rather than object to the application of the varnish, and they will often ask for its use more frequently than once a day. Instead of keeping the patients in bed and recumbent, my aim is to make them sit up from the first, and in fine weather I encourage them to walk out, if there be sunshine, the day after the operation, and I have never seen any ill consequences, but great advantage follows the adoption of this plan. There is certainly no worse practice than keeping the patients lying on their backs in bed, and I am sure that I have seen the evil consequences of doing so in several of my earlier cases. Under the above treatment the average time of convalescence is about seven days, and in uncomplicated cases no patient need be detained in hospital after this time.

It is only reasonable that I should state the grounds upon which I prefer excision of the tongue with scissors to its removal by the galvanic or any other form of écraseur. In the first place, I consider that it is more in conformity with the elementary prin-

ciples of surgery to make use of a sharp-cutting instrument, it being desirable to leave a clean surface—a surface which, it must be acknowledged, is in the best condition for healing. To employ an instrument whose purpose is effected by bruising leaves a surface in the very condition we are most anxious to avoid in the ordinary practice of aseptic surgery. The wound, in fact, left after the use of the écraseur embodies every condition calculated to engender those putrefactive changes which it should be our primary aim to avoid. I fail to see any special danger in the operation of excising the tongue which can justify such a departure from surgical practice, and I cannot recognise any good reason why we should employ an instrument for excising the tongue which no surgeon would at the present day ever think of making use of in any other region of the body—an instrument which almost every surgeon of eminence in the past has made a point of condemning. With scissors absolute precision can be obtained, whereas the erratic course of the écraseur wire is absolutely beyond the control of the operator. I am fully alive to the dangers which may result from blood gaining access to the lungs, even in small quantities, in patients with bankrupt constitutions; and although I am aware that some surgeons advocate a preliminary laryngotomy or tracheotomy, I believe that it is possible to guard against this danger by measures attended with much less risk than those incidental to either laryngotomy or tracheotomy. At any rate, my own cases conclusively show that better results can be obtained without than with these preliminary measures. It unfortunately happens that it is precisely in those cases in which the danger from blood getting into the air passages is the greatest, the risks incurred in opening the trachea are also at a maximum. In one of my own cases, in which I performed laryngotomy on the day previously to that fixed for excision of the tongue, the patient died during the night in consequence of the first operation. If the head of the patient is properly arranged during the operation, and if the tongue has been thoroughly detached as recommended, when the arteries are divided, the blood, unless the precaution has been taken to secure the vessels beforehand, will spurt directly out of the mouth, without any disposition to float backwards. We must also bear in mind that the entrance of blood into the air passages is only one, and not the most important, determining factor in the causation of septic lung mischief. Patients suffering from cancer of the tongue

with putrid discharges not unfrequently die from septic pneumonia apart from any operation or the entrance of any blood into the air cells, and, further, under an anæsthetic, large quantities of blood frequently finds its way into the trachea during the extraction of teeth; but how rarely do we meet with instances of septic pneumonia from such a cause. The skill with which the administration of the anæsthetic is conducted is also largely responsible for the amount of blood which may enter the trachea. For instance, as I have just stated, whilst it is desirable to have the patient thoroughly under the anæsthetic at the commencement of the operation, it is a mistake to give more than will just suffice to restrain his movements during the later stage. If this practice is adhered to, the patient's reflex power of guarding his own air passages is never altogether in abeyance, and no dangerous amount of blood will remain in the lungs at the termination of the operation, as it is practically completely expelled by the act of coughing. Take my sixty-six cases of simple excision of the tongue. In not a single instance were ill effects observed which could be attributed to the entrance of blood into the lungs—a record which tends conclusively to show that the tongue can be safely excised without a preliminary laryngotomy.

When we come to consider what we can fairly and reasonably claim to be gained by excising the tongue, we have two questions to answer, Does excision prolong life? Or, failing this, does it render the remainder of his life more endurable to the patient? From my own experience, I can bear the strongest and most emphatic testimony that in the majority of cases excision gives very great relief; and in support of this I could produce numerous letters I have received from patients and preserved for the purpose of answering this question. As to the prolongation of life, it is only necessary to turn to the statistics I produce, when it will be clearly seen what proportion of patients lived beyond the ordinary tenure of life after the advent of malignant disease of the tongue. It has been shown that twenty-six patients out of the eighty-four successful cases were traced, and found to have lived a year and upwards after excision; and I think that we are reasonably entitled to add to this number a certain percentage of the forty-one of whose fate nothing could be ascertained, and not to take it for granted that in every instance the patients had succumbed. Although cases unsuited for operation, and those where the disease has

recurred after removal are not exactly within the scope of my communication, I cannot avoid taking advantage of this opportunity to make a few remarks upon this much-neglected subject. We have it on good authority that in cancer of the tongue death generally takes place before dissemination has extended beyond the limits of the cervical glands—that, in fact, up to the very end the extent of the disease is within the range of eradication. Surgeons not unreasonably feel deterred from operating by the contemplation of the important vessels and nerves occupying the region traversed by the lymphatics, and it is no matter of surprise that so few efforts are ever made when the glands in the neck are invaded. In addition to this, patients have usually been reduced to such a degree of emaciation that by the time the glands are affected the prospects of recovery are very discouraging. If attempts are never made to follow the disease to its utmost limits, the possibilities of surgery can never be ascertained. I have on several occasions in desperate cases, and in response to the wishes of patients, cleared out the anterior triangle of the neck, and in two instances excised a portion of the common carotid artery, with, however, fatal results in both cases. This experience is naturally disappointing; nevertheless I have not altogether abandoned the idea that I may yet meet with more common results. In fact, I have recently operated upon a gentleman brought to me by Dr. Vaudrey of Derby, which has stimulated my hopes. The patient had his tongue excised in May last under most unfavourable circumstances, there being at the time extensive gland infiltration. The operation temporarily fulfilled the purpose for which it was performed, and relieved him from a foul mass of sloughing tissue within the mouth. He received so much benefit that he afterwards consented to have the glands removed, and this was partially carried out by another surgeon. The wound rapidly healed, but his sufferings were not entirely relieved by what had been done. At the request of Dr. Vaudrey I again saw the patient, and proposed as a means of relief that I should attempt to remove the tension which was apparently the origin of the pain. With this object, I removed the sterno-mastoid muscle from its origin to its insertion, and cleared out large quantities of diseased glands, and it appeared possible to have removed every visible particle had it not been found that the œsophagus was too extensively implicated to justify any further proceedings. During the course

of the operation the external and internal jugular veins had to be sacrificed, and the full extent of the carotid sheath exposed. The patient recovered from the operation, and lived absolutely free from pain for several months. It is an interesting fact, and worth mentioning, that the loss of his sterno-mastoid did not appear to occasion him the slightest inconvenience or in any degree interfere with the movements of his neck.

There is another subject in connection with cancer of the tongue which is constantly and painfully being brought under our notice, and that is, the singular view held by a large section of the profession that in cases of cancer the means of relieving pain are limited by the maximum dose of any sedative authorised by the British Pharmacopœia. It is a constant experience to find that a patient suffering incessant and intolerable agony is ordered the paltry dose of a grain of opium at bedtime, and it is the rare exception to find sedatives administered on lines proportionate to the amount of suffering. In contrast to this I may mention one of my own cases. A gentleman suffering from cancer arrived at the stage when he began to suffer pain, and he commenced with a quarter of a grain of morphia, which at first gave relief; by degrees this dose had to be gradually increased, until he eventually took as much as *thirty* grains three times daily. This kept him entirely free from pain, without at any time interfering with his intellectual interest in his daily surroundings. The total amount of morphia this patient consumed during the twelve months preceding his death amounted to 3512 grains. He also inhaled during the last four days of his life five pounds weight of chloroform, and my firm belief is that this patient would have died much sooner had not this sedative treatment been adopted from the first.

Tables which I have prepared show that out of 104 cases a history of cancer in the family was only obtained in six cases. A definite history of syphilis was obtained in seven, and a doubtful account in another seven cases. In sixteen patients the origin of the cancer was attributed to an injury. Sixty-one out of the 104 were ascertained to be smokers, and in the majority of these the cancer commenced on that side of the tongue on which the pipe was usually smoked. In thirty-three cases the irritation of teeth is recorded as being the exciting cause of the disease. In marked contrast to these cases, one man had never smoked in his life and

had not possessed a tooth for twenty years. The difficulties of ascertaining the duration of life after operation are very great. Patients, especially hospital cases, rarely remain for any length of time in the same house or even in the same locality. Mr. Wilson has succeeded in obtaining positive evidence in sixty-one cases; fifteen patients survived the operation one year; four, two years; two, three years; four, five years; and one, six years; and I may incidentally mention that the patient whose case I published in 1880 lived fourteen years, notwithstanding the fact that she was sixty-two years of age at the time of operation. If we believe that cancer of the tongue can be permanently cured by early excision, and that there are no other effectual means of eradicating the disease, we cannot too persistently urge these convictions upon those who have the earliest opportunity of detecting it, with the object of inducing them to adopt prompt measures and give the patients the only possible opportunity of having their lives saved. It cannot be too frequently reiterated that the loss of time in the interval between the discovery of the disease and its removal by operation bears a direct ratio to the death-rate, and conversely, that the earliest excisions give the most favourable results. The sooner patients can be made to realise how insignificant is the loss of their tongue compared with the loss of life, and that they have to weigh the two in the same balance, the quicker they will be to decide in favour of operation. This resolution will be taken with less reluctance when they are assured that the risks from excision are comparatively slight, and also that the inconveniences of being without a tongue are of small moment, as this organ is neither essential to speech nor to the sense of taste.

Probable Predisposing and Exciting Causes.—A probable family history of cancer was obtained in six cases out of the 104:—1. The patient's brother died from cancer of the tongue. The patient himself smoked cigars to excess, and there were indications of the tongue having been irritated by a tooth. 2. Patient's father died from cancer of the œsophagus. The patient himself was a smoker, and had carious teeth. 3. Patient's mother died from cancer. Patient was a moderate smoker. 4. Patient's uncle died from cancer of the lip. In this case there was no apparent exciting irritation—the patient did not smoke—the teeth had disappeared long before the onset of the disease. 5. Patient's mother's uncle had cancer of the lip. Patient smoked a clay pipe.

6. A cousin of patient's mother died from cancer. Patient had irritating teeth.

Syphilis.—Out of the 104 cases of cancer a definite history of syphilis was obtained in seven cases and a doubtful history in seven. In five cases actual syphilitic disease preceded the epithelioma. In one of these cases the ulcer was first brought on by irritation of decayed teeth, and subsided under treatment with iodide of potassium; three years later an epithelioma developed in the scar.

Exciting Causes.—Out of the 104 cases, in sixteen instances the patients distinctly traced the origin of the disease to some injury. 1. In five cases the tongue had been bitten. 2. In three instances the disease originated in dyspeptic ulcers. One of these patients was a glass-blower. 3. The repeated irritation of a simple ulcer with caustic gave rise to the disease in one case, and in another the scar left after the application of creosote was the site of the growth. 4. In five cases a definite history of repeated laceration of the tongue by carious teeth was obtained. This was well marked in the case of the youngest patient, a woman aged 25 years. 5. In one case the irritation of a plate of false teeth was the assigned cause.

Smoking.—In the 104 cases a note as to smoking or non-smoking is made in 79. Of these, 62 were smokers, of whom 41 habitually smoked clay pipes, and 13 are described as "very heavy smokers." In some cases it was reported that the disease began on that side on which the pipe was usually held. In 33 cases the existence of carious teeth is mentioned as well as the habit of smoking, and in the 15 cases in women 2 occurred in smokers.

Dr. PURCELL having tendered his thanks to Mr. Whitehead for his paper, stated that since Mr. Whitehead brought his operation of removal of the tongue, by the cutting method, before this Society in 1881, he, himself, had practised the removal after Mr. Whitehead's method, by means of the scissors; in his earlier cases he had, however, foliowed Professor Billroth's method, that of tying the lingual in the neck on the affected side, as a preliminary operation, taking advantage of extirpating any infected lymphatic glands at the root of the tongue through the same incision; later on, however, he dispensed with tying the linguals as a preliminary operation, with very good results. He differed in the way he placed his patient. After being anæsthetised, he brings the head towards the window and makes it hang over the end of the table; a good light enters the mouth, and any blood gravitates to the palate and not towards the epiglottis, and is therefore not so liable to be sucked down the windpipe. When disease implicates extensively the floor of the mouth,

and when he had found it advisable to divide the symphysis of the lower jaw, after Symes' method, and union after suturing the ends of the bone together with silver wire had occurred, he had found that the bone became diseased, caused it to soften, and break down the union, leaving the patient in a bad condition, so much so that he would not operate by this method, if any other offered. For such bad cases he had devised a method which he had styled "the hyo-glosso-epiglottidean," namely that of severing through the root of the tongue in a vertical plane by means of the galvanic écraseur, using one or at most two cells of a Grove's battery ; completing the removal of the tongue with scissors. It will be found, if tested by means of one's own finger, if introduced into the glosso-epiglottidean pouch in its deepest part, and another placed outside between the hyoid bone and the under surfaces of the angles and rami of the lower jaw, how little tissue intervenes between the fingers. The operation consists in passing through a submental incision a long curved needle on a handle armed with a silk thread, so as to land it in the glosso-epiglottidean pouch, first on one side, then on the other, and by means of these loop threads the wire, the ends being bent so as to form a hook, is pulled down and out at the submental space, landing the centre of the platinum wire over the root of the tongue ; using one cell of Grove's battery the wire is worked through, the root of the tongue is severed in a vertical plane, close up to the epiglottis ; the lateral and deep attachments are then snipped with scissors, the tongue released and withdrawn ; the operation is bloodless, a drain is put into the cavity and brought out at the submental incision. He practised feeding his patients the next day after operation, but always by means of a catheter or tube passed down the œsophagus, admitting food in a pulpatious form ; the patient relishes taking his food with confidence. Recurrence takes place, if not in the floor of the mouth, it soon attacks the glands of the neck and the patient dies from suffocation and exhaustion. Mr. Whitehead alludes to the contrasting the operation with the results obtained in ovariotomy ; this should be contrasted more with disease occurring in the os and neck of the uterus and the operations for its removal in this region.

Mr. Butlin said that he was sorry Mr. Whitehead objected to the comparison he had made in his book between the cases treated by Mr. Morrant Baker and Mr. Whitehead, on the ground that the operations differed greatly in severity. He believed that it mattered much less whether the operation included the whole or part of the tongue than whether the removal of a part of the tongue was complicated with removal of glands, or part of the jaw-bone or of a large part of the floor of the mouth. He had always found it very difficult to compare tables of operations on the tongue on account of the great variety of operations and of complications of operations. He objected very much to the performance of "indiscriminate operations," feeling sure that the best hope for the future lay in the careful selection of cases for operation, and that the greatest successes after operations for cancer of the tongue would follow on comparatively minor operations performed for disease still in a very early stage. For his own part, he wholly agreed with Mr. Bryant in condemning very extensive and severe operations for the removal of deep-seated glands ; for he had found that such operations afforded very little relief to the patient, and offered really no prospect of cure of the disease. With regard to the method of operation, he believed that Mr. Whitehead's operation was rapidly gaining ground in London : it had been largely practised by himself and his colleagues at St. Bartholomew's

Hospital during the last five years. He was in the habit of dressing the wound with powdered iodoform, and feeding the patient, if need be, through a tube for several days after the operation. He had come to regard the uncomplicated removal of the whole or a large part of the tongue as a comparatively trivial operation, for the mortality of late years had very largely diminished. Even when the glands were removed, the operation was usually borne exceedingly well. Of the final results of operations for cancer of the tongue, he could say that he was happy in being able to look back on several cases in which the patients were alive and free from disease three or more years after operation; but these were cases in which the disease was not advanced, and in which there was not any affection of the glands.

May 4th, 1891.

ANNUAL ORATION—ON THE COAGULATION OF THE BLOOD IN ITS PRACTICAL ASPECTS.

By Sir Joseph Lister, Bart., F.R.S.

Mr. President and Gentlemen,—Thirty years ago Dr. Alexander Schmidt, of Dorpat, enunciated a totally new view of the coagulation of the blood. Having rediscovered the fact observed many years before by Dr. Andrew Buchanan, of Glasgow,* that hydrocele fluid, uncoagulable in itself, is made to coagulate by the addition to it of the serum of blood already coagulated, and pursuing extended researches in the line thus indicated, he came to the conclusion that fibrin does not exist as such in solution in the plasma, but is composed of two albuminoid substances: one present in the liquor sanguinis, to which he gave the name of "fibrinogen"; and the other a constituent of the blood corpuscles, and this he termed the "fibrino-plastic substance."†

It might be objected to Professor Schmidt that the hydrocele fluid and the various dropsical effusions with which he had worked were not fairly comparable to liquor sanguinis; that they were transudations through the walls of vessels, and that the liquor sanguinis might have become in one way or another altered in the process of transudation. This objection was, as I believe, removed

* 'Proceedings of the Glasgow Philosophical Society,' Feb. 19th, 1845.

† 'Archiv für Anat. u. Phys.,' &c., 1861 and 1862.

by an observation made by myself about the same period.* It had fallen to my lot to observe that in mammalia, whereas the blood usually coagulates soon after death in the heart and the main vascular trunks, in the secondary vessels it remains fluid for an indefinite period, not only in those of small calibre, but, if the animal be large, in large vessels also. This being understood, I proceeded as follows:—Having removed a portion of the jugular vein of a horse with the blood contained in it, between two ligatures, I suspended this segment of the vein in a vertical position. In the blood of the horse the red corpuscles behave in a totally different manner as regards their aggregation from those of a healthy man or of the ox. In the horse, instead of the red corpuscles assuming the condition of a delicate network of rouleaux, they become aggregated into dense spherical masses, often visible to the naked eye, like coarse grains of sand. And these densely aggregated corpuscles, falling more quickly than the rouleaux in the liquor sanguinis, as hailstones fall more rapidly than snowflakes, soon leave the upper part of the fluid comparatively free from corpuscles, so that within about half an hour the upper third, or it may be half, of the blood is a transparent liquid.† When I had ascertained through the translucent walls of the vein that this state of things had occurred, I punctured the upper part of the vessel, so as to let out some of the clear fluid, and found that it was very slow in coagulating. In about three-quarters of an hour it had only begun to coagulate, whereas a little of this same clear fluid to which a small portion of coagulated blood was added clotted in a very short period. That the clear fluid did coagulate at all was sufficiently explained by microscopical examination, which showed that there were present in it some red corpuscles and numerous white corpuscles. It was obvious that if we could have separated the corpuscles absolutely from the liquor sanguinis, there would have been no coagulation at all; and as the separation of the corpuscles from the plasma had occurred, not by transuda-

* The Croonian Lecture by the author, on the Coagulation of the Blood, 'Proceedings of the Royal Society,' June 11th, 1863, p. 606.

† The same thing is seen in the mixture of serum and corpuscles obtained by stirring the blood during coagulation. In that from the horse the red corpuscles subside from about the upper third of the liquid within half an hour; whereas that from the ox shows only a thin layer of serum after the lapse of twenty-four hours. I am surprised to see that in some text-books the buffy coat is attributed to slowness of coagulation.

tion through vascular walls, but simply as the result of gravity, it could be no longer doubted that Schmidt's conclusions were essentially right.

During the time that has since elapsed various endeavours have been made to ascertain the precise nature and mutual relations of the constituents of the liquor sanguinis and the corpuscles thus concerned in the formation of the fibrin. This inquiry cannot be said to be yet terminated, and it is at the present time uncertain whether Schmidt's simple original view may not be correct—that there are two albuminoid substances, one in the plasma and one in the corpuscles, which combine to constitute fibrin. These investigations, most valuable as they are, have, as it seems to me, somewhat overshadowed the question which is, after all, the most interesting to us as practitioners—namely, What are the circumstances that determine the mutual reaction of these two constituents? What are the conditions under which the corpuscles are induced to give up their element of the fibrin, to combine with the element in the liquor sanguinis? This subject engaged a large share of my attention many years ago, and though I am afraid I have not much of novelty to communicate regarding it, yet, in consequence of its very great importance, I have thought that it might perhaps be not unworthy of the circumstances in which the kindness of your Council has placed me this evening.

Shortly before my investigations began, Professor Brücke, of Vienna, had conducted an inquiry into the conditions which determine coagulation, and had arrived at conclusions which, to a certain extent, resembled those to which I was led.* He found, as I also did, that there is a world-wide difference, in their relations towards the blood, between the walls of the living vessels and ordinary solid matter; and Brücke concluded, as Sir Astley Cooper and others had done before him, that this difference consisted in an active state of the living vessels; that the blood vessels, by an action which they exerted upon the blood, prevented it from coagulating. My investigations, on the other hand, led me to conclude that healthy blood has no spontaneous tendency to coagulate; and that the walls of the blood vessels are not active, as Brücke supposed, but passive in their relation to coagulation; that ordinary solids induce coagulation by an attractive influence —comparable perhaps to that which a thread exerts in causing

* 'British and Foreign Medical Review,' 1857.

the deposition of sugar-candy from a solution of sugar—while the healthy living tissues differ from ordinary solids in being destitute of this attractive influence.

Out of many experiments tending to this inference, published long ago,* I may be permitted again to describe one which not only appears to me conclusive on the point at issue, but is also of interest otherwise. A portion of the jugular vein of an ox with the blood in it being held vertically, I cut off the upper end, taking scrupulous care that the wounded part of the vessel did not come in contact with the blood, and then passed down into the vein a tube composed of very thin glass of a calibre rather less than that of the jugular vein, its upper end being stopped by a perforated cork, in which was inserted a narrow glass tube, which again was continued with a short piece of vulcanised indiarubber tubing. The tube was pressed with the utmost steadiness down into the vein, so as to disturb the blood as little as possible. In course of time, the vein being a little squeezed, blood made its appearance in the narrow glass tube and then at the end of the indiarubber tube. When this was the case, the indiarubber tube was secured by a clamp, and the whole apparatus was rapidly inverted and the piece of vein withdrawn. A piece of waterproof tissue was then tied over the open end of the large tube, to prevent evaporation and exclude dust, and the tube was securely fixed and left undisturbed. Thus we had blood present in a vessel consisting entirely of ordinary solid matter, but having been subjected in a minimum degree to the influence of the ordinary solid; and the result was that, when I came to examine the blood after the lapse of ten hours, I found it still fluid, with the exception of a crust of clot lining the wall of the vessel. This, gentlemen, seemed to me of itself to afford sufficient evidence that healthy blood has no spontaneous tendency to coagulate, requiring to be kept in check by an action on the part of the walls of the living vessels. This blood had been entirely withdrawn from the vein, and yet it remained fluid except where in contact with the ordinary solid.

This conclusion has been comparatively lately strikingly confirmed by the experiments of more than one observer. I would especially allude to one performed by Professor Berry Haycraft.† He has found that if a drop of blood is introduced under suitable

* The Croonian Lecture, 'Proceedings of the Royal Society,' *loc. cit.*

† 'Journal of Anatomy and Physiology,' vol. xxii, p. 582.

precautions into a deep narrow jar of castor oil, and before the drop, which falls slowly through this oil, has reached the bottom of the vessel, the jar is inverted and the drop made to retrace its steps without having touched the glass, this process being repeated again and again, the drop of blood, having never come in contact with an ordinary solid, remains fluid for an indefinite period. This experiment may, perhaps, appear to some of you even more conclusive than mine, inasmuch as no coagulation whatsoever occurs in the drop of blood under these circumstances. Certainly it seems to me that it confirms in an absolutely unmistakable manner the view to which I had been previously led.

But there is also this interesting circumstance in Professor Haycraft's observation. It had been shown amply by myself that the gases of the atmosphere are incapable of inducing coagulation of the blood; but experiments like those of Professor Haycraft show that the same is the case with neutral or chemically indifferent *liquids*. This seems to me to be an exceedingly interesting fact—viz., that the active living tissue, such as lines the wall of a healthy vessel, in its relation to the coagulation of the blood, resembles the mobile particles of a liquid.* I say the *active* living tissue, for when the living tissue becomes impaired in vital energy, it behaves towards the blood like an ordinary solid. That is the case not only when a vessel is wounded, but also when it is subjected to some influence which, without actually wounding it, is calculated to suspend or impair its vital activity. A good illustration of this is afforded by a fact which I have never before referred to, but which I have often noticed. A very valuable field for simple and instructive observations regarding the conditions that determine the coagulation of the blood was afforded by the feet of sheep, removed after the animals had been killed, the blood being retained in the vessels by a bandage applied below the part where the foot is removed by the butcher. The blood remains fluid for days in the veins of such feet; while, at the same time, the persistent vitality of the vessels is shown by the fact that they contract when exposed by reflection of the skin.† Now

* Dr. Freund, of Vienna, almost simultaneously and independently observed this relation of inert liquids to coagulation. 'Jahresberichte für Anatomie und Physiologie,' 1886.

† The blood in the amputated limb becomes gradually impaired in its coagulating property. A few hours after amputation it is found to clot more slowly

it happened that the butcher, in order to keep the sheep from struggling, always tied the feet together with a firm cord, applied below the part where my bandage for retaining the blood was passed round; and I invariably found the blood coagulated in the superficial veins at the part where they had been pinched between the cord and the bone. There is no reason to suppose that the temporary application of the cord had deprived the veins of their vitality at the part subjected to its pressure. If the sheep had been released, I have no doubt whatever that the veins would have remained alive. But though the vessels had not been wounded, but only squeezed, only had their vital energies temporarily impaired, nevertheless the blood had coagulated in them at the part so treated. Just as by pinching a portion of the web of a frog's foot with the padded ends of a pair of dressing forceps you can induce, by the irritation of mechanical violence, an intense degree of inflammatory congestion, in which the pigment-cells for the time being have their vital functions of diffusion and concentration of the pigment perfectly suspended, and yet are in a condition which is recoverable,* so did these veins, subjected to a similar agency, experience, though unwounded, a temporary prostration of their vital power. Thus it appears that the living tissues, which, while in a healthy active state, differ from ordinary solids in not occasioning the coagulation of the blood, themselves act like ordinary solids and induce coagulation when their vital energies are suspended.

Another point to which my investigations were at that time directed was the behaviour of the blood-blot in relation to coagulation. I came to the conclusion that, in a healthy state of the blood, an undisturbed coagulum resembles living tissue in its behaviour with regard to coagulation; that an undisturbed clot does not induce coagulation in its vicinity—a most important truth, if it be such. This is well illustrated by the fact with regard to the sheep's foot to which I have already referred. We have seen that, on the one hand, where the tight cord had pressed the veins, coagulation occurred in those veins; but that, on the other hand, the blood remained permanently fluid

than at first when exposed to the influence of ordinary solids, and after some days fails to coagulate at all.

* See a paper by the author on the "Early Stages of Inflammation," 'Phil. Trans.' for 1858, Part II, p. 682.

in other parts of the same vessels. In other words, the clot induced by the action of the cord upon the veins had not been able to spread, although the blood in the veins was perfectly at rest; the clot could not propagate itself. The same thing is seen in any amputated limb in which the blood vessels are sound. If such a limb is examined, say twenty-four hours after amputation, you will find that there exist clots in the vessels where they were wounded by the knife in the operation, but that elsewhere the blood remains fluid and coagulable. I came therefore to the conclusion that an undisturbed blood-clot is unable to induce coagulation in its vicinity; and I think that the instances I have given demonstrate that such is really the case.

Yet in the experiment which I have described, where a glass tube was slipped down into the jugular vein, the coagulum did propagate itself. I found, as before mentioned, on examining such a tube ten hours after it had been charged with blood, that the crust of clot which lined the tube was only a thin one; but in another experiment, nearly two days having been allowed to pass before examination, the layer of clot was thick, and there remained only a small channel in the middle of it, with the blood still fluid and coagulable. How is this difference of behaviour between the clot within the vessels and the clot outside them to be explained? At the time when I performed the experiment, I was disposed to think that it must be due to some imperfection in the mode of performing it; that in spite of all the care that I had taken in very steadily pressing down the very thin glass tube, nevertheless the blood must have been influenced by the glass for some considerable distance.

I am now inclined to believe that another explanation must be given. It was ascertained by Schmidt that (to adopt provisionally his original nomenclature) the fibrino-plastic substance emitted by the corpuscles is in excess of what is required for combination with the fibrinogen of the liquor sanguinis. This, in fact, is obvious from the fundamental truth that serum expressed from a shrinking clot, when added to hydrocele fluid, induces coagulation. The blood is coagulated already, the fibrin is already formed in it, and yet the serum contains fibrino-plastic substance in solution free to combine with the fibrinogen of the hydrocele fluid. Such being the case, we see that, in the experiment with the glass tube, the clot first formed under the influence of the glass,

shrinking and squeezing out its serum, containing fibrino-plastic substance, this must combine with the fibrinogen of the adjacent liquor sanguinis, forming fresh fibrin and producing a new layer of coagulum.

This, however, will only account for a very limited extension of the clot; inasmuch as the fibrinoplastic substance of the original coagulum having now all combined with fibrinogen, none will remain in solution, and thus the serum pressed out by the shrinking of the new layer will not contain fibrino-plastic substance, unless some new agency comes into play, to induce the corpuscles of the last-formed coagulum to give it up. Such an agency is, I suspect, present in the shrinking of the fibrin, which, as a matter of theory, seems quite as likely to have such an influence on the blood-corpuscles as the attractions of an ordinary solid. If such be the true state of the case, the clot must go on perpetuating itself indefinitely, however slowly.*

But how are we to explain the non-extension of the clot within the vessels in the cases referred to? How are we to explain the fact that it did not spread from the vicinity of the tight cord in the sheep's foot, and does not extend beyond the vicinity of the wound in the amputated limb? Again, why is it that the coagulum never propagates itself from the wound in the vein after phlebotomy? That wound, intentionally made somewhat gaping, is certainly in the first instance occupied by blood-clot. The equable flow of the venous blood does not disturb it. It is another example of an undisturbed coagulum. Now it must often happen that the clot projects more or less into the calibre of the vessel, in which case the blood at its lee side will lie at rest in contact with it. Yet the indefinite extension of the coagulum, which the analogy of the blood in the glass tube would lead us to anticipate, never occurs, and we reckon with confidence on the wound in the vein simply healing without interference with its calibre.

These remarkable differences between the behaviour of a coagulum in a glass tube and within the living vessels may perhaps have light thrown upon them by a fact which I have on a previous

* A good example of the indefinite extension of a clot outside the body is mentioned by Freund (*op. cit.*). He found that, while blood might be kept fluid for an indefinite period in a vessel completely coated with paraffin, if the coating was deficient at any point, coagulation took place there and spread throughout the mass.

occasion brought before the attention of this Society. I first observed it eighteen years ago, in an attempt that I made to obtain pure blood-serum from a horse, letting blood, with antiseptic precautions, from the carotid artery into a flask that had been purified by heating it to a very high temperature. To my great astonishment I found that, although the blood of the horse coagulated as usual in the flask, the clot did not shrink in the least. Though I kept it for many days, yet there was not a drop of serum to be seen upon its surface, and the sides of the clot remained in contact with the wall of the flask. This was made particularly striking by the circumstance that masses of aggregated red corpuscles, resembling grains of sand, as before described, were to be seen touching the glass in the lower part of the buffy coat. This, I need hardly say, astonished me immensely, and I imagine it was the first time that a blood-clot was ever seen not to shrink and press out serum.

It seemed hardly likely that this result could be due to the destruction of micro-organisms in the flask, although the heat had been applied for that purpose. It was suggested to me that, perhaps, it might arise from some physical change in the glass due to the very high temperature to which I had subjected it. It had been observed by Liebig that, whereas a super-saturated solution of sulphate of soda is, under ordinary circumstances, made to start into a crystalline mass by contact with a glass stirring-rod, no such effect is produced by the rod if it is heated in the flame of a spirit lamp, and allowed to cool—a result attributed by Liebig to some temporary physical change produced in the glass by the heat. Might it be, then, that the fibrin of the clot failed to shrink in consequence of a different molecular arrangement assumed under the influence of the glass altered by heat? That view, however, has been exploded, because it has been proved—as illustrated by some striking experiments shown at a conversazione of the Royal Society some years ago by my colleague Professor Thomson—that the cause of the crystallisation of the super-saturated solution of sulphate of soda is not the contact with the glass as such, but the accidental presence on the glass of minute quantities of sulphates isomorphous with the sulphate of soda; and that the effect of the heat is to drive off the water of crystallisation of those salts, and make them no longer isomorphous with it, and, therefore, no longer able to induce the crystallisation. And so a mystery in

physics was cleared away and made a very simple matter. Thus the suggested explanation fell to the ground.

The same absence of shrinking of the clot had been brought about by different means in the example which I exhibited to this Society nearly seven years ago.* A glass jar, not especially clean, had been purified by means of a solution of corrosive sublimate in 500 parts of water. Blood had been let into this jar from the jugular vein of a horse, under antiseptic precautions, forty-one days before; and the members of the Society had the opportunity of seeing that, just as in the case of the flask subjected to a high temperature, the clot had not shrunk; the serum had not been squeezed out of it. And although it seems unlikely that in the short time that elapses between the shedding of the blood and the commencement of shrinking of the clot, under ordinary circumstances, the micro-organisms present could have had such an influence on the blood, yet, when we see that two agencies so different in their nature as a high temperature and a solution of corrosive sublimate, but both powerfully germicidal, led to the same result, one is almost inclined to think that surely it must be so. No other explanation has been offered, although I know that physiologists have been much interested in the subject.

If we admit that micro-organisms are the cause of the shrinking of the clot, and that the shrinking of the clot is the cause of its extension, it follows that an undisturbed coagulum formed within the body under healthy conditions otherwise, being free from micro-organisms, will not spread. Fibrino-plastic substance exists, no doubt, in the serum in the substance of the clot. But the experiments of the late Mr. Graham showed that diffusion of liquids is an exceedingly slow process, even between a strong saline solution and water; and it must be a very slow process indeed between two liquids so nearly allied as liquor sanguinis and serum.

Whether this explanation be correct or not, the fact remains that an undisturbed clot of healthy blood within healthy living vessels is incapable of self-propagation.† On the other hand, the

* See 'British Medical Journal,' Oct. 25th, 1884.

† As further illustrations of this important truth may be mentioned the limitation of the coagula to the immediate vicinity of the wound in the veins of a stump after amputation; and also the fact, which I have often noticed, that if a varicose vein in the leg is treated by removing portions of the vessel at intervals of a few inches, the blood, though it coagulates in the immediate

theory which I have suggested is in harmony with the extension of clots containing microbes, as in pyæmia.

While an undisturbed clot resembles healthy and active living tissue with respect to coagulation, a disturbed and torn clot acts in this relation like wounded tissue. And as a coagulum is an easily lacerable substance, we often see coagulation induced by a blood-clot that has been disturbed. This is, perhaps, most strikingly seen in aneurysm. Let us take, for instance, a traumatic aneurysm. The blood escapes from the wounded artery into the surrounding tissues, and, these having been injured, the blood coagulates in contact with them. Every successive portion of blood driven in by the force of the ventricle stretches and lacerates the clot so formed. It is an injured clot, and induces coagulation in its vicinity; and the result comes to be that, while, on the one hand, the force of the heart tends perpetually to distend the sac, inferior as it is in elasticity to the wall of the artery, there is, on the other hand, a constant tendency to deposition of fibrin upon the interior of the sac, as when blood is "whipped" outside the body, and thus the wall of the sac is perpetually strengthened, exhibiting a counteracting agency tending to recovery.

A beautiful converse of this state of things is presented, as it seems to me, by some cases of varicose aneurysm, such as used to be a not uncommon result of careless venesection. Here the communication with the vein was sometimes so free that the blood driven in by the heart distended the sac comparatively little, so that the aneurysm had no tendency to increase, but was merely a source of annoyance from the purring sensation caused by the arterial blood driven forcibly into the vein. And if an operation was performed on such a case, the remarkable fact was disclosed that, instead of the aneurysmal sac being lined with layers of fibrin, it resembled in its interior an artery or a vein.* The force of the blood not being able to tell upon the clot and disturb it, the clot ceased to induce further coagulation, and became organised and invested with endothelium.

We see the same thing illustrated in the different behaviour of the blood-clot above and below a ligature applied upon an artery in its continuity. When such a ligature was applied in the old-

vicinity of the ligatures employed, remains permanently fluid in other parts of the vein.

* 'Syme's Principles of Surgery,' 5th ed., p. 140.

fashioned way in the shape of a silk thread, used without any antiseptic precautions, with the ends left long, it had to come away by suppuration, and there was serious danger of secondary hæmorrhage. But, if this did occur, it was commonly not, as might have been expected, from the cardiac side, where the seat of ligature was subjected to the full force of the ventricular contractions, but from the distal side, where the pressure was comparatively feeble. The ligature having been applied sufficiently tightly to rupture the internal and middle coats, there necessarily occurred as an immediate result a certain limited amount of coagulation upon the injured tissues. On the cardiac side, where the force of the blood, driven by the heart against the obstruction, told powerfully at every stroke, the clot was perpetually disturbed, and infallibly coagulation occurred up to the first considerable branch producing a substantial resisting plug. But at the distal side, if the circumstances of the anastomosing circulation were such that there was no pulsation, there might be no extension whatever of the undisturbed primary coagulum, so that a mere trace of clot was found on pathological examination.

I have, hitherto, dwelt on the view first advanced by myself, and now, I believe, generally accepted, that the fluidity of the blood is not due to an active operation of the living vessels. But I am far from holding the opinion that there is no part of the vascular system that actively opposes coagulation. If transfusion is practised in the ordinary manner, say by filling a syringe with blood and injecting it into the veins of the patient, the blood is subjected to the influence of an ordinary solid, which would inevitably induce coagulation within the vessels, unless there were some counteracting agency at work. It is astonishing how very short a period of contact with an ordinary solid determines the mutual reaction of the corpuscles and the liquor sanguinis. Yet no coagulation occurs as the result of such an operation. It is, I presume, in the capillary system that the correcting influence is exerted.

Again, when intense inflammatory congestion is produced by the application of some irritant substance to the web of a frog's foot, we see that the corpuscles, both red and white, adhere to one another and to the walls of the vessels, and block the capillaries. Mechanical violence is one of the many irritating agencies which produce such congestion; and from what we know of the effects

of the pressure of the cord upon the veins of a sheep's foot, we cannot doubt that the blood must be coagulated in the congested vessels; that between the corpuscles there must be cementing fibrin. The distinguishing characteristic of acute inflammatory exudation is of itself pretty clear evidence to this effect. The exudations in intense inflammation differ from those of dropsy by being coagulable. Hence the brawniness of tissues that are intensely inflamed, or the lymph in acute pericarditis. How can this coagulable character of the effused liquor sanguinis be explained except by supposing that the walls of the capillaries have acted for the time being like ordinary solid matter, and that, as a consequence of this behaviour of the capillaries, the corpuscles have given up to the liquor sanguinis (to use Schmidt's language) the necessary fibrino-plastic substance? And if the exuded liquor sanguinis coagulates, we cannot doubt that the plasma which remains in the capillaries is also coagulated. Nevertheless, if the irritant has not been pushed so far as to cause the death of the part on which it has acted, the tissues in due time recover, and we see the corpuscles gradually detaching themselves from each other to pass on into the circulation. And we may surely say that not only do the corpuscles recover their original non-adhesive character, but the fibrin which binds them together is dissolved.

I believe, therefore, that although in the larger vessels the vascular walls are negative as regards the coagulation of the blood, in the capillary system there must be potent agencies counteracting any tendency to clotting induced by abnormal conditions, and capable even of dissolving fibrin.

CLINICAL EVENINGS.

November 24th, 1890.

THREE CASES OF SPORADIC CRETINISM.

By H. Montague Murray, M.D.

Case I.—A boy, born in London; mother died of phthisis; three brothers died in infancy; cause unknown; other members of family healthy; nothing noticed till sent to school, then pronounced "backward;" no change last three years. Age 16; height, 3 feet 5½ inches; cranium, large, broad, flat on top; hair, coarse, scanty; face, large; eyes wide apart; nose, depressed bridge, flat, broad; lips, thick, purple, pouting; tongue, slightly larger than average, not protruding; teeth, so-called "double row;" skin, coarse, dry, never sweats except after diaphoretics; subcutaneous masses of fat in supra-clavicular regions; thyroid gland not felt; abdomen, protuberant; bowels, confined; limbs, short; hands, feet, and nails, broad, short, and thick; gait, waddling; circulation, normal; cannot read or write; good tempered, and is sent on errands; speech, slow, thick; urine, normal; temperature, normal. No signs of puberty.

Case II.—Girl, born and always lived at Hackney; parents and brothers and sisters known to be quite healthy; nine months child, weighed 10 lbs.; began to cut teeth when 2 years old. Age 6; height 2 feet 3 inches; cranium, large and broad, flat on top; hair, coarse, scanty; face, broad; eyes wide apart; nose, broad and flat; jaw, prognathous; lips, large and pouting; tongue, large and thick; skin, solid œdema, coarse and dry, no sweating ordinarily; subcutaneous masses of fat in supra-clavicular* and axillary regions;† thyroid gland not felt; abdomen, protuberant; bowels, confined;

* Since 4 months old. † This year.

limbs, short; hands and feet, broad; nails, small, irregular, furrowed; can neither stand nor walk; cyanosis, mottling of skin; mental state, infantile; speech, nil; grins when abdomen is tickled; approbation expressed by grunt; urine, normal; temperature, 97—98° F.

Case III.—Infant, born in Suffolk; parents known to be healthy, also one sister and father's mother; nine months child; snuffles at birth, no other evidence of syphilis; condition noticed during second month. Age 1½ years; height, 1 foot 11 inches; cranium, broad, flat on top; hair, coarse, scanty, extending over forehead; face, small; eyes wide apart; nose, broad, upturned, depressed bridge; lips thick, blue, large, pouting; tongue, *very* large and thick, generally projects; teeth, none; skin, coarse, dry, never observed to sweat; subcutaneous masses of fat, none; thyroid gland not felt; abdomen, protuberant; bowels, very confined; limbs, purple, enormous useless muscles, suggestive of pseudo-hypertrophic paralysis; hands and feet, slightly large, purple; cannot sit up; cyanosis and mottled skin; mental state, infantile; grins if abdomen is tickled; no grunt; urine, normal; temperature, 96° F. night and morning.

SPORADIC CRETINISM.

By J. Walter Carr, M.D.

Patient was a girl, æt. 4¼ years; both parents healthy; the mother had a fright from a dog during pregnancy. Two other children; both strong and well in all respects. Patient was born at full term, and has always lived in the neighbourhood of Clapham; she has never had any severe illness, and was first noticed to be backward and different to other children when 15 months old. At 12 months old she began to talk, and used to say a few simple words, but for the last four months she has not spoken at all; when about 16 months old she began to walk a little, but lately has quite gone off her legs. The child is stunted, and somewhat ricketty. Length, 28½ inches; circumference of head, 19 inches; weight 23 lbs.; skin, very dry, in places scaly, never perspires; subcutaneous tissues abundant, but the fatty tumours often seen in the posterior triangles of the neck in these cases are absent; neck, short; abdomen, very large and prominent;

limbs, short and thick; hands and feet, short, broad, and swollen, but do not pit; hair, dry and stiff, but fairly abundant; nails, normal; face, pallid and broad; nose, exceedingly flat; lower lip, thick; mouth kept shut; tongue not markedly swollen, and does not protrude; teeth, small, broken, and decaying; no swelling of soft palate; thyroid, no trace can be felt; knee-jerks present, plantar reflexes very slight; bowels very costive, and have been since birth; urine, average sp. gr. 1016, scanty, no albumen. The child sits up, but does not attempt to stand; feeds herself, but never speaks, and takes no notice of her surroundings beyond smiling at times. Temperature varies between 97° and 99° in the rectum. The hands and feet are sensitive to cold, and the hands especially readily turn blue. The case is one of distinct, though not of extreme, sporadic cretinism, and is rather interesting in that the intellectual condition seems at one time to have been somewhat better than at present, and the retrogression has not been associated, as in some recorded cases, with any acute illness.

TWO CASES OF CLEFT PALATE AFTER OPERATION.

By Edmund Owen, F.R.C.S.

Mr. Edmund Owen showed two boys, aged 7 and 9½ years respectively, on whom he had operated a few weeks previously for closure of wide clefts of the hard and soft palates. In the case of the elder boy, the cleft had extended from the incisor teeth to the tip of the the uvula; but in the younger one the hard palate had been involved only in the posterior two-thirds. He had operated in each case with the boy's head hanging down over the edge of the table, chloroform being administered by a Junker's inhaler. The chief feature of the operation was that after approximating the long lateral flaps of muco-periosteum, he had completed the relief of the tension of the soft palate by adopting Billroth's plan* of detaching the lower part of the hamular process. By this means the course of the tensor palati was shortened, and that muscle, together with the palato-pharyngeus, was slackened. In the elder boy's cleft, seven sutures of silver wire and four of horsehair had been employed. In each case, the cleft of both hard and soft

* 'Centralbt. für Chirurg.,' March 29th, 1890.

palate was found completely and soundly closed, the only part in which union had failed being the end of the uvula. He was not prepared to say as to how far the favourable results obtained were influenced by the adoption of Billroth's suggestion.

CASE OF PRECOCIOUS MASTURBATION.

By C. E. Beevor, M.D.

Charles B—— aged 11 months. Between 4 and 5 months old mother noticed that he was always working his legs about, and thereby producing an erection of the penis; this has continued ever since, and at times his mother has noticed a glairy fluid exude. Takes hold of the penis with his hand at times.

Family History.—Mother's father paralysed after a fit; no insanity. Two other children (girls) healthy. No cause can be ascertained to account for the condition. He has been circumcised last June without effect. Mr. Allingham has sounded for stone, but without result. The urine is faintly alkaline, but no albumen; no phosphates; no worms been passed.

When the child is prevented from moving the legs about he manifests discomfort.

TWO CASES OF CONGENITAL TUMOURS.

By John H. Morgan, F.R.C.S.

Mr. John H. Morgan showed a patient of his from the Hospital for Sick Children, with a small tail-like projection from the sacral region. The child was a girl aged 7, and had also a small exostosis from the posterior surface of the right os calcis. The "tail" was a short projection of skin, about three-quarters of an inch long, standing out at right angles to the back. It was not compressible, and was placed a little to the left of the middle line over the centre of the sacrum. No deficiency of the arches of the sacral vertebræ was to be felt, nor was there any evidence of arrested development of the spinal cord, but the cellular tissue around its base was fuller than natural.

Note.—The projection was afterwards removed, and, though it

had no deep connections, the spinal canal at the corresponding spot appeared to be closed by membrane only.

Mr. John H. Morgan also exhibited an infant, perfectly formed and well nourished, but having a hard, non-compressible tumour situated between the buttocks and attached by a broad base rather to the left of the middle line and beyond the extremity of the coccyx. In size it was about that of a child's fist, and at the apex a small area of fluctuation could be felt, but the remainder was very firm in consistence, and was only slightly movable. On examining per rectum two distinct swellings containing fluid were perceptible to the finger. They were situated at the posterior surface between the gut and the sacrum; oblong in shape and separated by a narrow band from each other, and extending upwards to a level with the top of the sacrum. Nothing like bone or cartilage could be distinguished in the body of the pendulous mass, and its exact nature could not be diagnosed, but it seemed to greatly resemble other similar tumours of a congenital nature which consist of fœtal remains.

CASE OF HYPERTROPHY OF LOWER LIMB IN A CHILD, NOT CONGENITAL.

By W. B. Hadden, M.D.

The patient was a little boy, aged 6, who had been under Dr. Hadden's observation for more than two years. When he was about 4 years old it was noticed that the upper and front part of the left thigh was becoming swollen, and the swelling gradually extended downwards to the foot. There was nothing in the history of the case or in the appearance of the limb pointing to lymphatic obstruction. The entire left lower limb was affected, but the thigh relatively rather more so than the leg. The enlargement seemed to be due to an increase in the subcutaneous fatty tissue. There was no deformity of the limb. Examined apart from its fellow, it might be passed as natural. No bony or muscular hypertrophy was present. There was no increase in muscular power. The knee-jerks were normal. No abnormality of vessels could be detected.

	July 13, 1889.	Nov. 22, 1890.
	inches.	inches.
Circumference of left thigh four inches above patella...	14¼	14
,, ,, right ,, ,, ...	10½	11¼
,, ,, left leg two inches below patella	9¼	9¼
,, ,, right ,, ,, ...	7½	8

The left foot was about three-quarters of an inch more in circumference than the right. It would appear from the above measurements that the left lower limb had practically remained of the same size during the last sixteen months, whereas the right lower extremity had undergone some increase, but probably not more than could be accounted for by the natural growth of the limb. The lower limbs were of the same length. There was no accumulation of fat about the buttocks, which were natural and of the same size, nor was there enlargement of any part of the body except the left lower extremity. The boy enjoyed good health, and had suffered from no special illness.

CASE OF CONGENITAL DEFORMITY OF THE LEFT HAND.

By James Black, F.R.C.S.

The patient was a healthy boy, aged 4 years, presenting the following deformity of the left hand. An entire absence of the index and middle fingers: a proximal phalanx alone developed in the ring finger, the latter being webbed to the little finger, which was apparently perfectly developed. The thumb was normal.

Mr. Black showed the case to elicit an opinion from the Fellows as to the advisability of dividing the web, and said he himself felt inclined to adopt the procedure recommended by Mr. John Morgan of detaching a dorsal and a palmar flap from the alternate aspect of the two affected digits, so as to guard against the edges of the divided web becoming reunited.

A CASE WHERE, TWO YEARS BEFORE, ONE-HALF THE TONGUE HAD BEEN REMOVED FOR EPITHELIOMA DEVELOPING UPON OLD LEUCOPLAKIA; NO EVIDENCE OF RECURRENCE.

By A. Marmaduke Sheild, F.R.C.S.

The patient was a man aged 73. He came under treatment in December, 1888. For twenty years he had suffered from a "bad tongue." There was no evidence of syphilis, but the patient had been an inveterate smoker, using large quantities of strong tobacco, and clay pipes. For nine months a growth had been manifesting itself on the right half of the tongue. It was about the size of a hazel nut, hard, warty, easily bleeding, and intensely painful. The whole of the surface of the tongue was found cracked, and covered with a hard, white epithelium, of leather-like consistence. The glands were not affected, but there was marked emaciation. Removal of the whole organ was refused. Removal of the right half of the tongue only was therefore performed. After removal the growth showed clear evidence of epithelioma under the microscope. The greatest local and constitutional relief followed the operation. At the present time the remaining half of the tongue was in the same condition as above described, and it seemed likely that epithelioma would there develop. The case was brought forward to illustrate the local origin of the varieties of epithelioma, and how satisfactory were the results of early removal of them. Especial attention was drawn to the difficulty of early diagnosis, and doubts were expressed as to the accuracy of microscopical examination. "Cell-nests" were by no means pathognomonic of epithelioma as was commonly believed.

CASE OF ABDOMINAL PULSATION.

By T. Gilbart Smith, M.D.

Dr. Gilbart Smith showed a man, aged 27, a blacksmith by trade, who, thirteen years ago, was seized with a severe attack of vomiting and gastric pain after food; he did not, however, have any symptoms like those premonitory of gastric ulcer. For five

years the attacks ceased, but eight years ago they again recurred, and lasted from three to seventeen weeks. The man had lost flesh, and found that the attacks were more severe when he was at work. He had never brought up blood, and there was no microscopical evidence in the vomit of gastric ulceration. In the abdomen there was very distinct pulsation, though now less marked than formerly. Dr. Gilbart Smith did not believe that aneurysm was present, and he diagnosed simple neurotic vomiting. The treatment adopted had been washing out of the stomach, which had been attended with benefit.

CASE OF RUPTURED URETER.

By Herbert W. Allingham, F.R.C.S.

Mr. H. Allingham showed a patient on whom he had operated for rupture of the ureter. A blow from a barrow-handle had been received on the left side of the abdomen, and the patient on admission to the hospital was in a state of collapse. Three days after admission the abdomen was explored, and a large, sausage-shaped swelling found in the region of the ureter, which was thin and tense. He tapped this from the loin, and a pint and a half of turbid fluid was withdrawn. Drainage was maintained for a fortnight, and thus the wound healed. Before the operation the amount of urine had diminished, whereas afterwards it increased in quantity.

A CASE OF GRAVES' DISEASE.

By A. Ernest Sansom, M.D.

Dr. Sansom showed a case of Graves' disease, with pronounced retraction of the upper eyelids (Stellwag's sign). There was very slight descent of the upper eyelid with the downward movements of the globe (well marked Von Graefe's sign). Patient was a female aged 24. The concurrent signs were:—Palpitations, pulse rate usually about 100; thyroid enlargement (slight, chiefly left lobe); tremors (especially arms), restlessness, pigmentations of skin (chiefly face, folds of axillæ, groins, and popliteal regions). Amenorrhœa. A soft systolic murmur was heard at heart-apex, and venous hum in neck. Anæmia not marked.

The order of evolution was probably (1) periodic sweatings and flushings (several years); (2) palpitations (about $3\frac{1}{2}$ years); (3) appearance of prominence of eyeballs, said to have been first noticed on left eye (8 months). The exophthalmos is more apparent than real, very little prominence is noticed when eyelids are closed.

The interest of the case is chiefly in the question of the causation of Graves' disease, "Why the retraction of the upper eyelid?" On this point evidence wanted.

A CASE OF ABSENCE OF PULSE IN ARTERIES OF RIGHT UPPER LIMB.

By Sir Hugh Beevor, Bart., M.B.

Sir Hugh Beevor showed a case in which no pulse could be felt in right subclavian and upper limb. George W——, aged 45. Intemperate for some ten years in earlier life, with no history of former illness or syphilis, and unconscious of anything unusual in connection with the right arm. He presented signs of degenerative changes in a marked degree: anus senilis, slight emphysema, much diminished liver, dulness and a trace of albumen in the urine. The heart's apex in the nipple line with heaving impulse, but no murmur, and the first sound weak at the apex. Pulse 96, full and well sustained, with occasional intermittence. The right arm felt less warm than left, and was one-tenth less in girth, both upper arm and forearm, the artery could be felt at the wrist but not the pulse. Over the upper part of the manubrium sterni the breathing was tracheal. Veins were apparent over upper part of chest. Sir Hugh Beevor proposed that the cause of this condition was not congenital but acquired, and suggested that it was due to a small aneurism in preference to endorteritis or tumour.

CASE OF PARALYSIS OF THIRD NERVE RECURRING EVERY FIVE MONTHS.

By H. R. Rivers, M.D.

The patient aged 13. Since he was 5 years old the following sequence of events have recurred every five months regularly. He

is seized with headache and vomiting, followed by the signs of paralysis of the third nerve, which latter last about two months, then improve, the improvement never being quite complete, and five months later the headache and vomiting recur, and the history is repeated. He has also a mitral presystolic murmur, but is in other respects quite healthy.

February 16th, 1891.

CASE OF RAYNAUD'S DISEASE.

By F. de Havilland Hall, M.D.

W. M.——, aged 31, labourer. The patient had been under Dr. Hall's care for pulmonary phthisis. In the beginning of January he noticed what appeared to be a frost-bite on the patient's left ear, and, on examination, found the right ear similarly affected. The patient told him that the first attack occurred in December, 1889, when the ears turned blue, but there was no pain in them; the fingers and great toes also became of a bluish-black colour. The present attack began in December, 1890, and the ears were very painful, but the hands and feet were not affected. The necrosed part of the helix of the ear was commencing to be thrown off. No pain since the patient had kept the ears covered up, and taken five minims of tincture of opium thrice daily.

CASE OF ADDISON'S DISEASE.

By F. de Havilland Hall, M.D.

V. D——, aged 35. Night attendant at a cab stand. The patient was first seen in the out-patient room on December 1st, 1890, when he said that he had been suffering from vomiting for a week. Dr. Hall noticed that the lips, tongue, and buccal mucous membrane were much pigmented. The patient was ordered bi-carbonate of sodium in infusions of calumba, and, a fortnight later, he said that he had only been sick once since the previous visit. There was no return of the sickness for five weeks,

but on January 23rd he again vomited, felt unwell, and took to bed. He was admitted an in-patient under Dr. Sturges on January 26th, 1891. As regards family history, the only fact of importance elicited was that his father (a Frenchman) died at the age of 50, of consumption. The patient was born, and has always lived, in London. When 5 years old he had a fall, resulting in angular curvature of the spine, but had no serious illness until he began to vomit, seven weeks before. There was nothing abnormal to be detected in the lungs and heart, and he did not appear to have lost much flesh. The face had become darker, and there was an increased amount of pigment about the nipple and in the axillæ. On the legs and arms there was a pigmented rash. The staining of the mucous membrane of the mouth was of a bluish-black colour.

CASE OF THYROID GLAND.

By F. de Havilland Hall, M.D.

Harriet E——, aged 53. Dr. Hall exhibited the patient, in whom enormous masses of what appeared to be thyroid gland were to be seen on both sides of the neck, with greatly dilated veins over the chest. Twenty-one years ago a cyst on the right side of the neck was tapped, and a seton put in; there was great increase in the growth afterwards. She had had no treatment for the throat since. She complained of nothing abnormal, except some little difficulty in swallowing fluids, and some dyspeptic symptoms.

CASE OF COMPLETE TRANSPOSITION OF VISCERA.

By F. de Havilland Hall, M.D.

H. B——, a porter, aged 46, whom Dr. Hall had shown to the Society on February 7th, 1876, as an example of complete transposition of viscera. Since then the patient had had three attacks of rheumatic fever, and his heart had become further damaged. In addition to the conditions pointed out when last shown, Dr. Hall drew attention to the systolic murmur, heard loudest in the second left interspace, and conducted to the sterno-clavicular articulation; and to the diastolic murmur, heard loudest over the sixth

right costal cartilage, and conducted towards apex. The apex was 3 inches below the right nipple, and 1 inch to the right of the nipple line.

CASE OF GANGRENOUS STRANGULATED INGUINAL HERNIA.

By Stephen Paget, F.R.C.S.

Mr. Paget showed a man, aged 24, on whom he had operated for strangulated left inguinal hernia. Finding the gut gangrenous, he fastened it outside the abdomen and opened it at once. The man did well, the bowel skinning over and receding, till there remained only a minute fistula from which a drop of mucus exuded once or twice a week. Mr. Paget urged that it was not justifiable in such cases to perform primary resection or to adopt Helferich's suggestion of making an anastomosis above the gangrenous bowel.

CASE OF ASTHMA IN A BOY AGED 10.

By W. P. Herringham, M.B.

Dr. Herringham showed a boy, aged 10, who was the subject of asthma. When 3 years of age the boy had suffered from scarlatina, and after that he had pneumonia and swollen feet. Dyspnœa also developed, which had since become more frequent and severe, and was worse in summer. The attacks were typical of spasmodic asthma, ending with a severe coughing fit, in which he brought up a quantity of muco-pus. There was no sign of bronchiectasis, though the boy was suffering from emphysema and bronchitis. He had found that the compound lobelia powder when burnt and inhaled as the dyspnœa came on, completely relieved the attacks.

CASE OF AMPUTATION OF BOTH MAMMÆ FOR SCIRRHUS.

By Edmund Owen, F.R.C.S.

Mr. Edmund Owen showed a married woman, aged 49, who came into St. Mary's Hospital last October with scirrhus of each

breast. On neither side was the skin adherent, or the breast attached to the pectoralis. She had had a child ten years ago, but there had been no trouble with either breast, nor was there any history of injury. Two years before her admission she had first noticed a hard lump growing in the right breast, and a year and a half later a similar growth on the left side. The growths, though of no great size, were extremely hard and nodular. Under an anæsthetic, he removed the right breast, whilst his house surgeon, Mr. H. J. Foster, amputated the left. Each axilla was also cleared of enlarged glands. The patient showed no particular shock after the operation; the wounds healed by first intention, and her present condition was eminently satisfactory. It was formerly taught that operative treatment was contra-indicated when both breasts were the seat of malignant disease, and in the last edition of 'Erichsen's Surgery' that teaching recurred. To this, however, Mr. Owen took exception. In the early stages cancer was a local disease. The fact of one breast only being almost invariably the seat of the disease was evidence of its local nature. When both breasts were affected, the condition might be regarded as accidental. It by no means followed that the bilateral occurrence indicated constitutional implication; and the fact of both breasts being scirrhous might actually be a double reason for advising prompt operative interference.

CASE OF DEXIOCARDIA.

By W. Ewart, M.D., and W. H. Bennett, F.R.C.S.

Dr. Ewart and Mr. Bennett showed a case of dexiocardia, without transposition of other viscera, and bronchiectasis, in which pulmonary incision and resection of the right ninth rib had been done for the latter. The patient was a groom, aged 28, who had suffered from cough since his infancy, and latterly had expectorated much pus. Three years ago he suffered from hæmoptysis, and last year from influenza and bronchitis. In July he had high fever with pneumonic signs at the right base, followed next day by the development of pneumonia on the left side also. The heart on examination was found to be out of position, and at the right base later there was pectoriloquy and ægophony. The base of the right lung was explored, and a drainage tube left

in for a week without evacuating any pus. At the present time the pulmonary symptoms remained the same. Dr. Ewart was unable, from the examination he had made and from the history of the case, to decide whether the displacement of the heart was congenital or due to the traction exerted by the contracting right lung.

CASE OF TREADLER'S CRAMP.

By J. Hughlings Jackson, M.D., F.R.S.

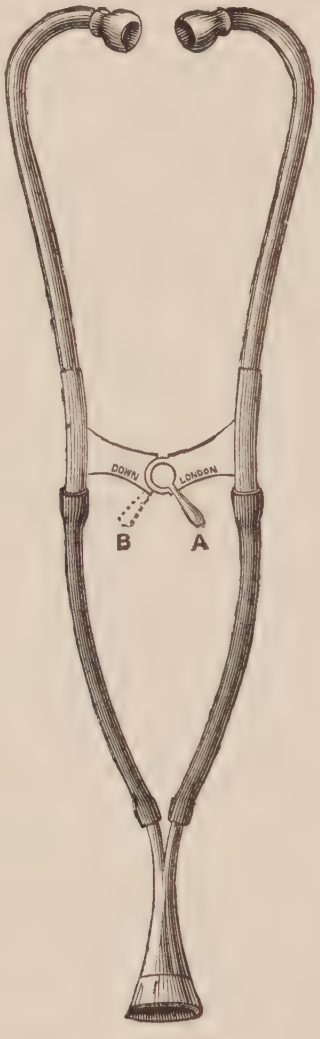
Dr. Rivers showed for Dr. Hughlings Jackson a case of treadler's cramp occurring in a man who, after having been a hand-loom weaver for thirty years, began to make mistakes in his work owing to defective treadling with the right leg (the one principally used); later the right leg became lame, and after using the left leg for some years this became weak, rendering him unable to follow his occupation. The spasm occurred at the commencement of the flexion movement which accompanied the upward motion of the treadle, the extension or downward movement being well performed. The spasm was of the combined movement of the hip and knee, each joint being moved freely by itself. The difficulty was referred by the patient to the gluteal region, and both the gluteal and hamstring muscles on the right side showed decided diminution of faradaic and galvanic irritability. The right leg was held stiffly in walking, the case thus agreeing with other occupation spasms in which large movements were concerned, and in which the affected limb was more or less generally disabled.

NOTES ON AN IMPROVEMENT IN BINAURAL STETHOSCOPES.

By George A. Herschell, M.D.

The most troublesome part of an ordinary binaural stethoscope is the spring. If it is strong enough to press the instrument well in contact with the ears it is painful, and if the spring is made weaker it fails to keep the ends of the apparatus accurately adjusted to the auditory canals. I have endeavoured to overcome this defect by substituting for the spring a clamp which will fix the hinge in

any position in which it is placed. By this means the user is enabled to adjust the instrument to exert just as much pressure upon his ears as he wishes, and then fix it in that position. With a little practice it can be used with the same facility as those furnished with a spring, and is so constructed that it can be re-



moved from the ears without touching the screw. The difference in construction from an ordinary stethoscope consists merely in the substitution of a screw furnished with a tail-piece for the rivet which usually connects the two arms, and which forms a hinge between them. This tail-piece is actuated by the second finger of the right hand, and enables the arms of the stethoscope to be instantaneously fixed at any angle.

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